PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXV.

PART II.

LONDON,

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PHILOSOPHICAL

TRANSACTIONS.

X. *On some phenomena of colours, exhibited by thin plates.* By John Knox, *Esq.* *Communicated by the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.*

Read April 6, 1815.

It is not without reason that the phenomena of light have been subjects of speculative enquiry from the earliest ages of philosophy; since perhaps no study can be more interesting to the inquisitive mind, than the contemplation of that medium through which it receives its most exalted enjoyments.

There are probably no appearances in physical optics that have excited more attention, and that have been less satisfactorily accounted for, than those prismatic concentric rings which appear between lenses, or between a flat glass and a lens, when laid together. And, notwithstanding some of the most eminent philosophers and opticians have given explanations thereof, particularly Sir Isaac Newton and Dr. Herschel, it will appear from what follows, that the subject is far from being exhausted.

The insufficiency of Newton's theory for the solution of this problem, by the supposed fits of easy transmission and

reflection of the rays of light, is now generally admitted; nor does it appear that any other more satisfactory, has yet been adopted in its stead.

This will not be thought surprising, if it shall appear by the following experiments, that neither NEWTON, nor any other writer that has followed him in the same line, has been in possession of all the phenomena connected with this curious and intricate subject.

I was induced to make the following experiments in consequence of having lately read Dr. HERSCHEL's excellent paper on the same subject, published in the 95th vol. of the Philosophical Transactions, in which he has related a great number of experiments, explaining a variety of phenomena relative to prismatic rings; and must acknowledge my obligations to this celebrated author, for his simple but ingenious contrivance of viewing prismatic phenomena by means of the shadow of a black card; without which, it is probable the following discoveries, such as they are, would not have been made.

Exp. 1. On repeating some of the experiments mentioned by Dr. HERSCHEL in vol. 95 of the Philosophical Transactions, and having by means of the shadow of a blackened card perceived no less than eight sets of rings in some cases, and being very attentive in endeavouring to distinguish transmitted from reflected sets, I faintly saw parallel lines or streaks, which at first were mistaken for the threads of a piece of black silk which was placed under the lower glass, in order to see the rings more distinctly. It was not until after repeated examinations by the light of a lamp, in which the direct light is shaded from the eyes, that it was ascertained,

that those parallel lines were drawn through the intersections made by the several rings of the primary set and its reflected image, and that they consisted of light, although their dimensions were too small for their colours to be distinctly perceived.

The apparatus used in this experiment was a piece of good looking-glass plate laid on the plane side of a plano-concave lens belonging to a compound microscope, and was used for holding mites or other animalcules on the concave side. The plane side of this glass had acquired in the polishing (accidentally perhaps) a very small degree of convexity, probably equal to that of a lens of several feet focal length. This lens, when the plane glass was laid thereon, produced a larger set of primary rings than could be otherwise procured. Its concave side had been ground on a sphere of about two inches radius; therefore the set of transmitted rings, reflected from its lower internal surface, was too small to cause any confusion either in the primary set or its reflected image; and it was in a great measure owing to this accidental circumstance, that these parallel lines happened to be discovered at all; nor could they have been discovered, even with this apparatus, without the use of the shadow.

Exp. 2. Having ascertained the reality of these parallel fringes, I painted the concave side of the plano-concave lens black, in order to prevent all reflection from its concave surface; by which means the fringes were seen in much greater perfection. They were found to consist of all the prismatic colours; were equidistant and parallel; equal in number to the rings of both sets taken together, exclusive of the central one; and each fringe was drawn through the several inter-

sections of the primary set of rings with those of its reflected image; and their lengths extended to the edge of the lens on both sides, they were formed at right angles to the direction of the light, and to a line joining the centres of the primary and its reflected image; which indeed is a necessary consequence of their being projected through the intersections of two sets of concentric rings perfectly equal in dimensions. See fig. 1, pl. VII. compared with fig. 3, pl. VIII.

I was convinced that those parallel fringes consisted of prismatic colours; yet with the apparatus now used, they were too small and too close to each other to enable me to perceive distinctly the order in which those colours were arranged, with respect to each other. As their distances from each other, or, which is the same thing, their breadths depended on the distances of the intersections through which they were projected; it was obvious that by separating, or widening the distances of the latter, the parallel fringes would also be separated, and consequently their breadths enlarged.

Exp. 3. For effecting this purpose, two modes presented themselves, either by increasing the dimensions of the primary set of rings, and consequently that of its reflected image; or by lessening the distance between them. The former mode was at that time out of my power, having no lens of a longer focus than the one in use. The latter was effected by procuring a thinner piece of plate glass than that used in the 1st experiment; and although by this apparatus the field of view was narrowed, the breadths of the fringes were enlarged, and by this means it was easily perceived, that each fringe was composed of the same prismatic colours as the Newtonian rings,

and were placed in the same order with respect to each other; that is, beginning with red, next orange, then yellow, green, blue, indigo, and lastly, violet: but that the fringes themselves were divided into two classes, (with respect to the position of the colours of each particular fringe) by a central band or fringe passing through a point bisecting the distances between the centres of the primary set and its image, each fringe, on both sides of this imaginary point, respecting it as a centre, and having the red colour of each fringe turned *outwards*, or from that centre; the central band alone being equal on both of its sides with respect to colour; being as it were composed of two of the inside halves of the two adjoining fringes, imagined to be laid together; having no red in its composition, and being rather less intense in colour than any of the others. An imitation is given in fig. 1. pl. VII. and as it would be impracticable to express all the colours of each fringe by shadowing, the darkest shade denotes the violet, the unshaded side the red of each fringe, and the black lines the divisions between them.

Exp. 4. Another mode of producing those fringes was by applying a convex and a concave lens together, the curvature of the one differing but little from that of the other. This apparatus produced a larger set of primary rings, and consequently broader fringes, than by either of the foregoing experiments; but in this case none of them were straight, except the central fringe, all the rest being bent more or less into elliptical shapes, conformable to the surfaces between which they were formed.

Exp. 5. But a better mode still was found, by applying a slip of looking-glass plate to one of the sides of a triangular

prism, about $5\frac{1}{2}$ inches long, and a full inch broad; for although the sides had been intended to be made perfectly flat, they had acquired, in polishing, such a small degree of convexity, as on application of the flat glass plate, produced a larger set of primary rings than could be had by any of the former experiments. This apparatus was still farther improved, by painting two of the sides of the prism black, so as to exclude all extraneous light; by which means I could easily perceive that those fringes (which were produced by rings of about three quarters of an inch diameter,) extended from one end of the prism to the other, that is, to an extent of at least seven or eight times the diameters of the rings, and it is uncertain how much farther they might have extended with a longer prism. They were also seen parallel to the edges of the prism, by carrying the eye in that direction, either to the right or left.

Nor do these rings or fringes cross each other undisturbed, for the prismatic colours of the rings, where they intersect each other, are deranged from their natural order; and the division lines of the fringes, where they intersect the other intersections, are drawn into a zig-zag form, but beyond those intersections they proceed in straight lines. These several intersections produce a most beautiful appearance of chequer, or rather net work, the meshes of which assume the hexagonal form, resembling the cups of a honeycomb; of which it is impossible to convey an adequate idea without seeing the experiment.

Exp. 6. Having discovered unexpectedly these phenomena from one set of primaries and its reflected image, I was induced to try what effect could be produced by two sets of

primaries brought into a similar situation with respect to each other. For this purpose, a double convex lens, of about thirty-six inches focus, was laid on a flat piece of looking-glass plate, having its under side painted black; on the lens was placed another piece of plane glass plate; by these means two sets of primary rings were produced, whose positions with respect to each other could be varied at pleasure. On using the shadow of the black card I was agreeably surprized to find, that instead of parallel *fringes* a new species of prismatic *rings* appeared, whose number and sizes varied with the positions and distances of the two sets of primaries; their dimensions were from two to three times the diameters of the primaries from which they appeared to originate, sometimes only one set appeared, sometimes two, and at other times a third very faintly.

On first observing these new rings, it was found, that on moving the eye in a horizontal direction to the right or left, they sometimes moved *with*, and sometimes contrary to the motion of the eye; others were stationary, although the eye moved; also, that sometimes the prismatic colours were seen in the usual order, and at other times inverted; all of which facts seemed not a little perplexing at that time; but their causes will be better understood from what follows.

Exp. 7. By a subsequent experiment, it was discovered, that those rings towards the circumference of the new sets had their colours always in the usual order; but that those nearest the centres had their colours always inverted; that the number of rings of each class were equal: that they all passed through the several intersections of the two primary sets of rings with each other, from which intersections they

seemed to originate; that the dividing ring between the classes, passed through a point, whose distance from the centre of each primary set was in proportion to its largest diameter.

This will be better understood by referring to fig. 2. pl. VII. where A and B are the primary sets of rings: C will represent one of the newly discovered sets, which were denominated *intersectionaries** from their apparent origin. The fourth ring from the centre will be the division between the two classes. Those rings within the division having the red on their *insides*, and those without having the red on their outsides, as represented by the figure; where the same rule has been observed in shadowing these rings, as was observed with respect to the parallel fringes in fig. 1, namely, that the shaded side represents the violet, and the unshaded side the red of each ring, and the dark lines the divisions between them.

Admitting that parallel fringes are necessarily rectilinear, in consequence of being drawn through intersections of circles that are perfectly equal in dimensions, it follows, that where two sets of circles differ in dimensions, the corresponding intersections cannot lie in straight lines, but must necessarily be circular, as will appear evident on inspection of the figure; where the dividing ring D E could not pass through the several intersections in the points 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, unless it were circular: the same thing will hold true of all the other rings of the intersectionary set.

It also appears by the foregoing experiments, that rectilinear fringes and intersectionary rings are coloured exactly alike, and are alike divided into two classes with respect to the order of the colours. There is also an exact similarity in

* The best apology for using a new word is, that it expresses a new idea.

the reticulated crossings before mentioned, whether the sets of primaries by which they are caused, are equal or unequal in dimensions. Fringes and intersectionary rings, therefore, differ in no other respect than as right lines and circles; it is, therefore, a fair conclusion, that the phenomena of rectilinear fringes, formed between two sets of rings of equal magnitudes, are bent into rings when the two corresponding sets are unequal in magnitude.

There is also an infinite variety in the dimensions of those intersectionary rings, according as the diameters of the primaries differ more or less, being *least* where that difference is *greatest*, and increasing in size as the two sets of primaries approach to equality, until at last they end in straight lines. The dimensions of these intersectionaries will also (*cæteris paribus*) diminish as the two sets of primaries approach each other, and enlarge as these are separated; and, like Newtonian rings, enlarge or diminish with the less or greater elevation of the eye. It will be easy to conceive from inspection of fig. 4, (pl. VIII.) that where an intersectionary set of rings is formed between two sets of primaries of unequal magnitudes, it must necessarily appear on the side towards the smaller of the two primaries; which agrees with actual experiment.

The above hypothesis accounts for one set of intersectionaries; but experiment shows that if one set appears, it is almost always accompanied by a second, sometimes equal, but oftener unequal in dimensions, as in fig. 3, (pl. VIII.) at other times part of a third set appears as segments of circles only. It would, therefore, seem that they are formed not only between primary sets, but also between primaries combined with either

transmitted or reflected sets, provided the two, between which they are formed, are unequal in dimensions.

Exp. 8. The four sets of rings represented in fig. 3, (pl. VIII.) were produced by an arrangement consisting of two convex lenses laid on each other, and a piece of plane looking-glass plate laid on the uppermost lens. The large imperfect set, whose centre is at D, was evidently produced by the intersections of the two primary sets, A and B, (the former was observed to have a dark and the other a light centre), but the set whose centre is at C cannot be accounted for on the same principle; but may be owing to another cause, which will be better understood by a future experiment.

Notwithstanding these intersectionaries seem to owe their origin to a few rings only, adjoining that which divides the two classes; yet they are always filled up to the centre with prismatic colours; unless when very large, as that whose centre is at D, (fig. 3. pl. VIII.) or when they appear in part only, as segments of circles; in which case the central spaces appear blank.

Exp. 9. It is perfectly certain that these intersectionary rings are always formed between the two contiguous surfaces of the two lower pieces of glass when three pieces are employed; this is easily proved by the test of the shadow. But if four or more pieces are laid on each other, the number of spectra may be increased indefinitely, and these again may be multiplied by internal reflections; so that to pursue these phenomena through all the varieties of which they are capable would be an endless task.

Let it be observed here, that the several drawings of pris-

matic rings in figures 1, 2, 3, (plates VII. VIII.) are such as would appear to the eye, could it see them at right angles to the plane in which they are formed; but as the eye cannot well see them in this position, nor until it is lowered to an obliquity of about thirty degrees, the proper allowance must be made; for such rings will always appear of elliptical forms, less or more elongated according to the greater or less elevation of the eye. See fig. 4. (pl. VIII.)

Exp. 10. Finding from the foregoing experiments, that two sets of rings of equal magnitudes produced straight fringes, and that those of unequal sizes produced rings; it was obvious that both fringes and rings might be considered as diagonals to the angles in which they were formed. It was, therefore, concluded from analogy, that if primary fringes could be produced between two flat pieces of looking-glass plate, and that if two of those sets were made to cross each other, a set of straight or rectilinear fringes should be formed diagonally between them; and, on making the experiment, I had the pleasure to find the result precisely answered expectation. So far theory and experiment agreed (see fig. 6, pl. VIII.); but in the course of making this and some of the succeeding experiments, several unexpected phenomena were discovered.

Exp. 11. The slips of plate glass used in this and several of the following experiments were from four to six inches long, and about one and a half inch broad, their edges having been ground straight.

On wiping two of those glass slips perfectly clean with soft shammy or a soft dry linen rag, (which is absolutely necessary, as the smallest particle of dust, scarcely perceptible even with a microscope, interposed between the slips,

would prevent the success of the experiment), and by applying two of their ends together, and by using some friction and a considerable degree of pressure, a beautiful set of rectilinear prismatic coloured fringes appeared across the glass slips, having all the prismatic colours in the same order as Newtonian rings, and equally vivid, (see fig. 5, pl. VIII.) and although the breadths of those fringes could be increased or diminished by the greater or less degree of pressure (which had the effect of diminishing or increasing the angle formed by the planes), yet their breadths continued to be uniform, or as nearly so as might be expected, considering the unavoidable imperfections of the best plate glass.

It was proved by Sir ISAAC NEWTON, that primary prismatic rings were repeated at equal increments of interval between two spherical surfaces, because the semi-diameters (or, which is the same thing, the diameters) of those rings were found on actual measurement to be to each other as the *square roots* of the series, 0, 2, 4, 6, 8, &c. beginning at the centre of the rings or point of contact, as must be well known to mathematicians; it might, therefore, have been known by reasoning *a priori*, that if similar prismatic phenomena could be produced between two flat planes, the corresponding repetitions must take place at regular and equal intervals from the point of contact, or vertex of the angle, corresponding to the natural numbers of which the measurements of the rings express the square roots; that is, as the series 0, 2, 4, 6, 8, &c. The principle is, therefore, fully confirmed by this experiment.

Exp. 12. Those primary fringes have also transmitted sets between them, alternate in colours to their primaries, exactly similar to Newtonian rings; for, by placing them longitudi-

nally in the direction of the light, and by using the shadow of a black card thrown across them, both primaries and transmitted fringes may be seen at once by the naked eye. Let the light come in the direction AB in fig. 5, pl. VIII. and let CD represent the edge of the shadow next the light, those fringes above the line CD are primaries, and those below it are transmitted sets, alternate in colours to the others. Although these primary fringes are uniform in their breadths, they are not quite so with respect to colour; for the yellow, blue, indigo, and violet, are predominant in the first, second, and third fringes next to the place of contact, although these four colours are but little perceived after the third fringe, where ten are seen, the red, orange, and green being predominant in all the rest.

Exp. 13. After having produced one set of primary fringes, I succeeded in having another set formed at right angles thereto, by applying a third slip of glass longitudinally to the upper one of the first two, on which the expected diagonal fringes immediately appeared in the angle between the two primary sets, as shown in fig. 6, pl. VIII. where B and C are the primary fringes, and D the intersectionary set divided into two classes, as shown by the dotted line.

Exp. 14. It was found by trials, that the relative position of two sets of fringes, when formed by three slips only of glass plates, connected together as in Experiment 12, could not be changed at pleasure; but, by using four slips, and having a set of fringes formed between each pair, which were unconnected, these could be placed the one over the other in any position, and at any required angle; by this arrangement, it was discovered, that whatever was the magnitude of the angle

formed by the two sets of primaries, that angle was always bisected by the central band of the intersectionaries. And although these latter evidently proceed from the crossings of the two sets of primaries, yet they are never continued through those crossings to the opposite angle at A, fig. 6, (pl. VIII.) nor could they be made to appear in any angle formed by primary fringes, unless the said fringes were so disposed as to have their red sides turned inwards, or towards each other. This remarkable fact was proved by several repetitions of the same experiment.

Exp. 15. In the course of making these experiments, I had accidentally left a single slip of glass on one of the pairs between which a set of primaries were formed, without any other pressure than its own weight; on examining the apparatus a few minutes afterwards, I was agreeably surprised to find no less than four sets of the same kind of fringes which appeared in the angle between the two sets of primaries, as before related in Experiment 12, almost parallel to each other, and nearly so to the primaries, and at about an inch distant in front thereof. (see fig. 5. pl. VIII.) The set marked 2, had broader fringes, more vivid in colour, and consisted of a greater number, (from fifteen to nineteen), than those sets on each side thereof; for these latter appeared to have that inferiority of colour to the principal set, that it bore to the primary set. All the four sets were by attentive observation visible to the naked eye, and were all divided into two classes by a central band, as was before observed of the others; and on application of the shadow, were found to consist of primary and transmitted fringes, precisely in the same manner as first primaries, or Newtonian rings.

A representation is shown in fig. 5, (pl. VIII.) before referred to, where the sets denoted by 1, 2, 3, and 4, may be called primaries in respect of 5, 6, 7, and 8, which, in like manner, may be denominated transmitted sets; all the space below the line CD being supposed covered with a shadow of a black card.

Not having hitherto observed these secondary fringes, except where they proceeded from intersections of primaries, it was concluded, that this was the only cause of their appearance; but, by this last experiment, I was convinced of the error of this opinion, and that they are entitled to the same rank of originality as primary fringes or Newtonian rings; and, since they are always found divided into *two* classes, I shall henceforward venture to denominate them *binaries*, which perhaps may not be an improper characteristic for the whole genus, in contradistinction to *primaries*, as it will appear in the sequel, that there are several species of the former, and at least two of the latter.

Exp. 16. This experiment was made to ascertain whether two slips of glass only, when the uppermost was pressed by its own weight alone, would produce binary fringes between them; and I succeeded in perceiving one set whose breadths were about one-tenth of an inch each, see fig. 9, (pl. IX.); these are best seen by clear day light; but it is also necessary to have a piece of black velvet under the lower slip, otherwise to have its under side painted black.

Exp. 17. In endeavouring to see these fringes mentioned in the last article by candle light, I was disappointed, but accidentally perceived others of a different species, less in breadth, but much more numerous, and by which the whole length and breadth of the glass slips were covered. These were visible

by the light of a small candle only, nor could they be perceived except in the image of the blaze of the candle reflected from the upper surface of the upper slip of glass, and by moving either the eye, or the candle, or the slips across the direction in which the fringes appeared: and even with every precaution, I sometimes failed in seeing them, which I attributed to the presence of very minute dust between the glass plates, which in some cases can be known only by its effects. Moreover, when the eye changes from a greater to a less quantity of light, it requires some time to adapt itself to such delicate and minute objects, before it can perceive them satisfactorily. See fig. 8. (pl. IX.)

Exp. 18. By subsequent trials it was found, by the use of an oil lamp, having five or six small wicks in a row, composed of hempen packthread, which produced a pale brownish light, that those singular fringes could be seen with nearly as much ease and certainty as any of the other phenomena, though not all at once; but, by moving the light over them, otherwise moving the slips with respect to the light, and by using a magnifying glass, it was ascertained that the specimen represented by fig. 8, (pl. IX.) contained thirty of these fringes to an inch. Being uncertain whether they might not be the same as the primary fringes mentioned in Experiment 11, or a continuation of them, I caused a few of these latter to be formed at one end of the slips at A, fig. 8, (pl. IX.) when it was found that they crossed each other at a certain angle as shown in the figure; therefore, they could not be the same. Moreover, as they have no perceptible colour like first primaries, nor are divided into classes as binaries, they must be different from either.

Exp. 19. It next occurred to try what effect could be

produced by three slips of glass when laid together, and pressed by their own weight alone. Leaving them in this situation about fifteen minutes, I found an irregular set of binaries spread over the surface of those slips; a representation of which is shown in fig. 9. pl. IX.

On drawing or sliding the upper piece of glass along the surface of the middle one, these fringes changed their shape, and disappeared as far as the middle slip was uncovered by the upper one; or if the upper two were kept together, and both moved over the surface of the under slip, the same phenomena took place. But if the upper glass was in the smallest degree separated from the other two, or if the two upper pieces were in the same degree separated from the lower one; in either case, the spectra first changed their shapes and then vanished; but, on leaving the slips to the pressure of their own weight, were again as instantly restored. On changing the position of these three slips by placing the two upper ones across the lower one, a new spectrum was formed as seen in fig. 10, pl. IX. In these as well as in the following figures of binaries, the central band, dividing the two classes, is always denoted by a dotted line.

These spectra may be varied almost to infinity by the smallest change of distance or relative position of any one of the three slips to the other two; and it affords a pleasing amusement to observe those fleeting forms start into new and fantastic shapes in such a manner as strongly to resemble, in miniature, the coruscations of the aurora borealis. In order to determine whether these spectra were formed between the two upper, or between the two lower surfaces of the glass slips, the test of the shadow was applied, which determined

them to be always formed between the two lower contiguous surfaces.

Exp. 20. But it appearing very unaccountable that the mere presence of the upper glass slip should produce spectra between the two lower slips; I wished to have the fact corroborated by another experiment. For this purpose two similar slips of glass were so closely applied together, as to produce primary fringes between them; and in this position they were cemented together with bees' wax, to prevent shifting: this double slip being substituted, instead of the two upper unconnected slips in Experiment 18, produced binaries as usual. On application of the shadow, the primaries appeared in the second, and the binaries in the third shadow, as expected; but it being perfectly certain that the primaries were formed between the two upper contiguous surfaces, and as the shadow proved that the thickness of one slip of glass was interposed between the two spectra, there could not remain a doubt of the binaries being formed between the two lower contiguous surfaces.

Exp. 21. These results being so unexpected with three slips, it naturally occurred to me, to try what effect four would produce. Having placed two, laid together horizontally on a table, I took up the double slip mentioned in Experiment 18, in order to clear it of dust, and was in the act of holding it up between the light and the eye, in order to examine whether it was perfectly clean; and it happening accidentally to be in the direction of the two lying on the table, I was agreeably surprised to find a spectrum already appear, as if formed in the air, although the two pairs of slips were several inches asunder; which spectrum, on moving the

double slip in the hand, out of the direction of those on the table, vanished as far as those on the table were uncovered by those in the hand. And notwithstanding this spectrum appeared to be formed in the air, the test of the shadow proved it to be formed between the two slips on the table; and although it was visible when the two pairs of slips were not less than ten inches asunder, it could not be perceived except through the medium of the double slip held in the hand.

A representation of this experiment is shown in fig. 11, pl. IX. where AB is a section of the two slips held in the hand; and CD of those on the table. Fig. 12 pl. IX. represents one of those fleeting and fantastic forms as seen by the eye at F in this experiment.

Exp. 22. If the result of the last experiment appears extraordinary and unaccountable, the present one will appear still more so; for, on lowering the double slip in the hand from its former elevation of about 45 degrees, to about 15 degrees, so that its image was seen reflected from the upper surface of the upper glass on the table, as represented by GH in fig. 13. pl. IX., a new spectrum, fig. 14, pl. IX. was seen in this reflected image, superadded to that shown in fig. 12, which was not to be perceived in the object A B itself! Both spectra together, as seen by the eye at F, are represented in fig. 15.

Exp. 23. It was fully ascertained by the test of the shadow, that while the double slips were in the position described in Experiment 22, both spectra were formed between the two slips on the table: but, on lowering the two upper slips until they were laid flat on those incumbent on the table, both

were gradually metamorphosed into two others; one of which was formed between the two lower, and the other between the two middle slips brought into contact by this new arrangement; this was also proved by the test of the shadow. It may be worthy of remark, that in every variety of shape assumed by these spectra, the binary characteristic is clearly visible in them all.

Exp. 24. On viewing a set of Newtonian rings, as described in Experiment 4, through one of the double slips, while in the position fig. 11. pl. IX, the apparatus forming the rings being on the table; a set of narrow binary rings appeared, concentric to the primaries, and near to the edge of the lenses: and on lowering the double slip to the second position, fig. 13. pl. IX., another set of binary rings appeared in the reflected image of the double slip, also concentric to the primaries, but consisting of broader rings, though less in diameter than those of the first set; and all these three sets were seen distinctly, at one and the same time.

The two sets of binary rings, seen in this experiment, correspond to the two irregular spectra in experiment 20; and since the rings must have been formed between the lenses on the table (because no *rings* are ever formed unless between surfaces, one of which at least is spherical), this affords a corroborating proof, that the irregular spectra seen in Experiment 20, although apparently formed in the air, were really formed between the two slips of glass lying on the table.

The above results differ so widely from any that have hitherto been published, that it is allowable to doubt, whether they can be accounted for on the common and received

Fig. 1.

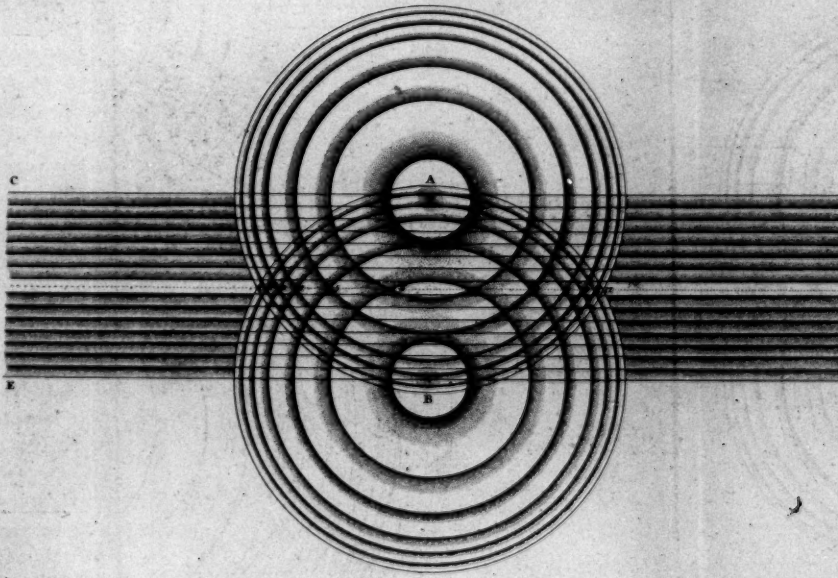
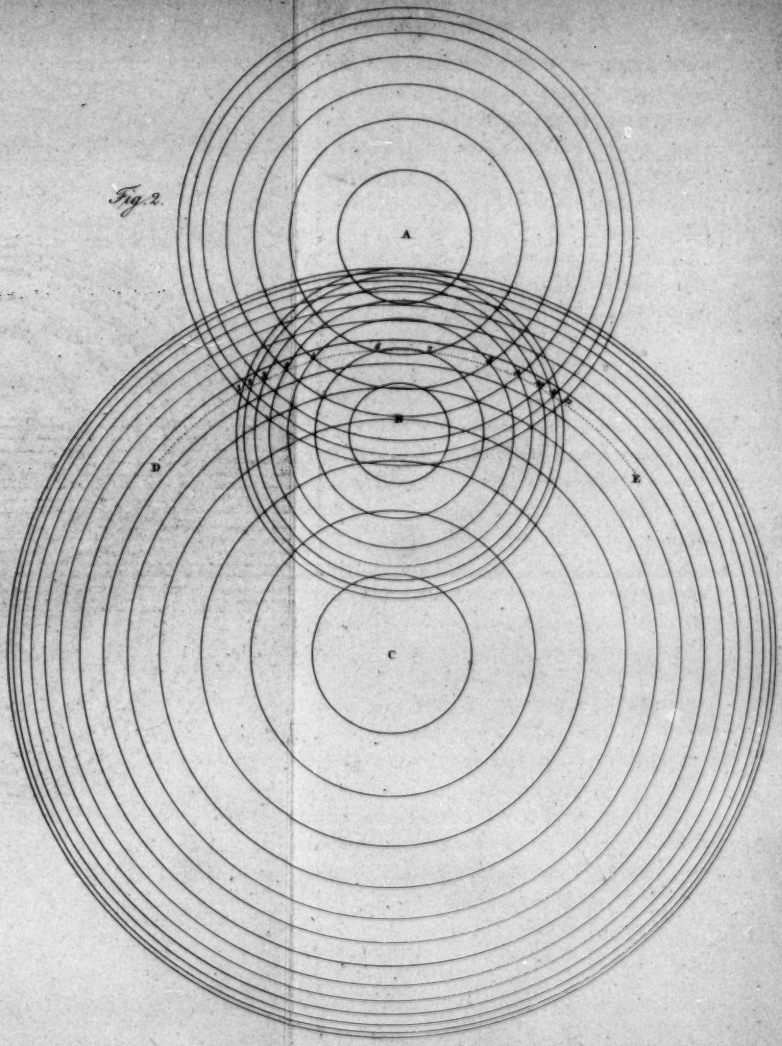


Fig. 2.



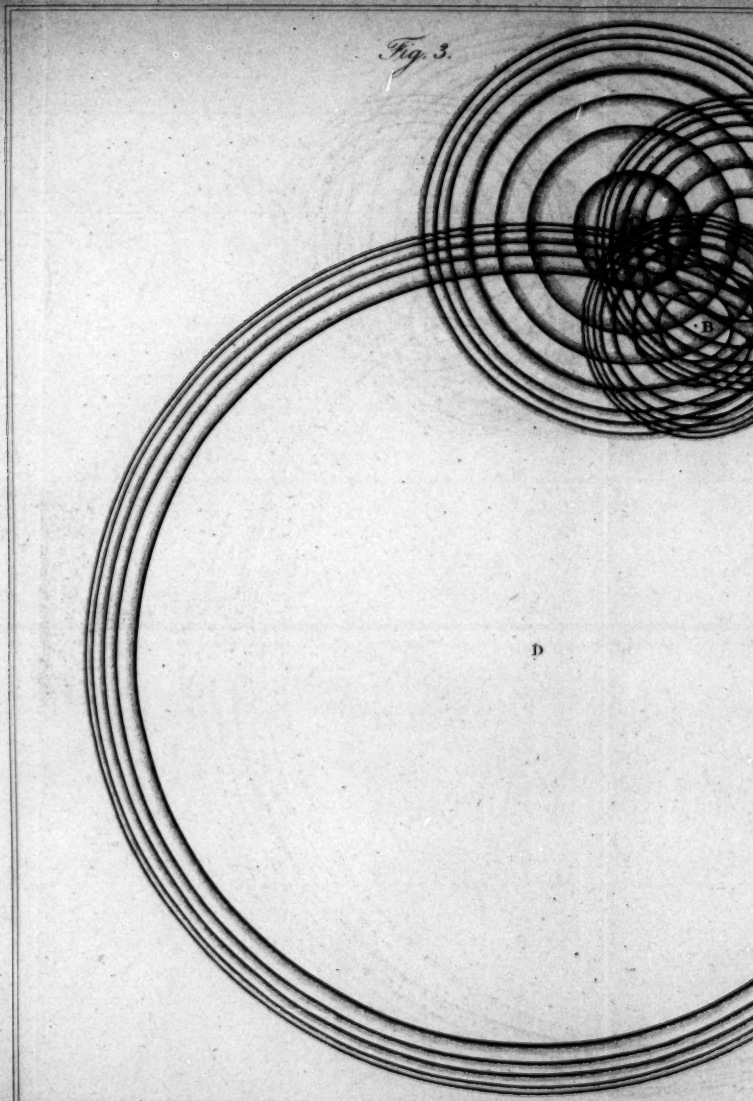


Fig. 3.
B
D

Fig. 4.

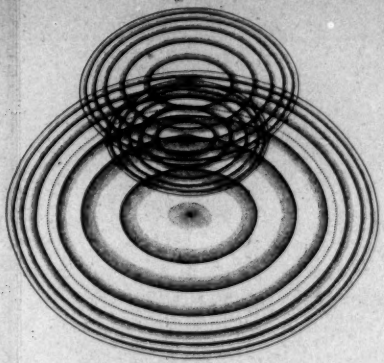


Fig. 5.

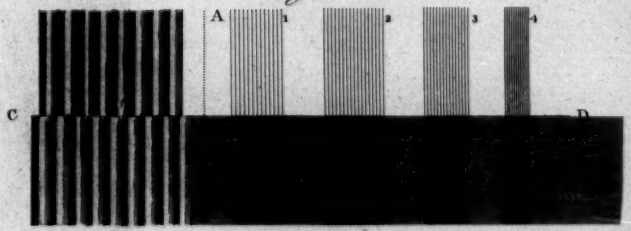


Fig. 6.

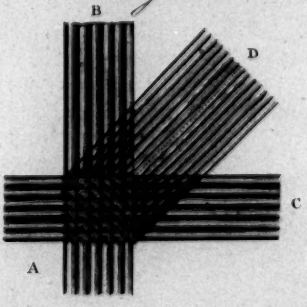


Fig. 7.

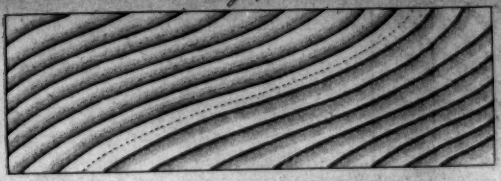


Fig. 8.

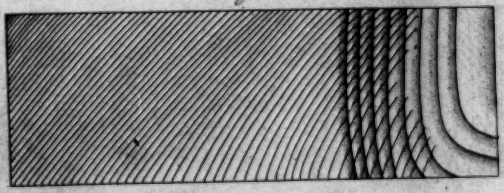


Fig. 9.

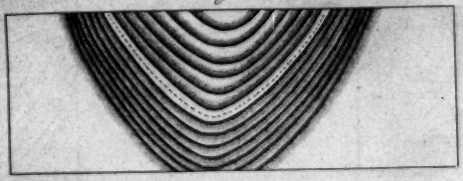


Fig. 10.

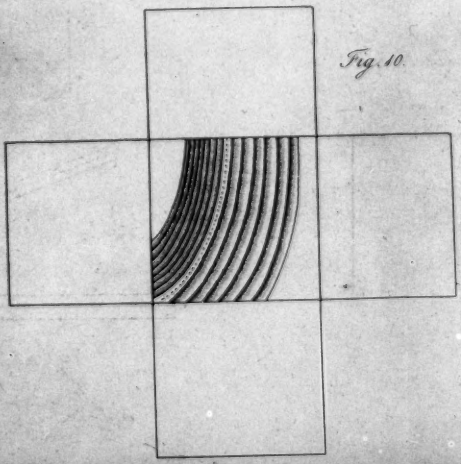


Fig. 11.

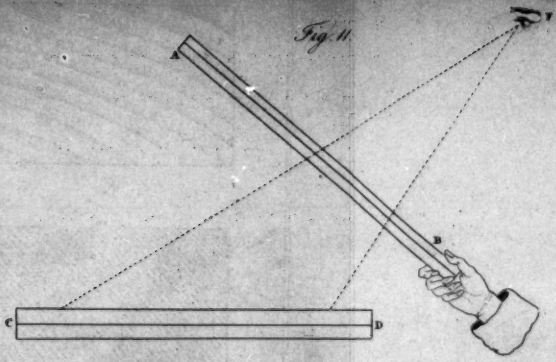


Fig. 14.

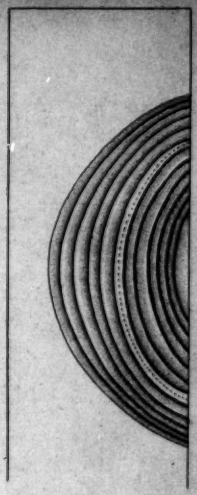
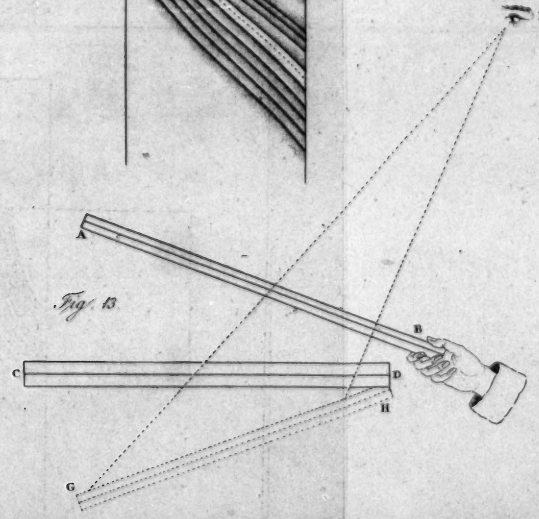


Fig. 12.

Fig. 15.

Fig. 13.



principles of physical optics, unless perhaps the newly discovered principle, which has been denominated the *polarity* of the rays of light, may serve to explain their causes. However, "as every desideratum may be considered as an imperfect discovery," which when completed may lead to others of still greater importance; and as the subject is so far from being exhausted, that perhaps we are only entering on a new career of discoveries, not only in optics, but in every other branch of natural knowledge; in this point of view, the prospect before us is rendered as extensive as it is animating and delightful.

JOHN KNOX.

Belfast, Oct. 6, 1812.

XI. *Some farther observations, on the current that often prevails, to the westward of the Scilly Islands.* By James Rennell, *Esq. F. R. S.*

Read April 13, 1815.

During the interval of 21 years, since the Society did me the honour to receive my Observations on the Current to the Westward of Scilly, more facts relating to that current, have been collected; as well as observations on its effects, in different parts of its course, between Cape Finisterre and Scilly: the whole tending to a confirmation of the general system set forth, in 1793; and, in one instance, affording, perhaps, a clearer proof of the strength of the stream, in respect of its *northerly* direction, than any of those, adduced on the former occasion.

In pursuing the detail of these facts and observations, I shall begin, in the neighbourhood of Cape Finisterre, and proceed with the course of the current, along the Bay of Biscay; and thence, across the mouth of the British Channel, to Scilly, and the entrance of St. George's Channel.

The three first facts, regard the current from the open sea, setting into the south side of the Bay of Biscay, and along the north coast of Spain; which current has been supposed, in the former Paper, to be occasioned by the prevalent westerly winds; which force the water near the shore, *into the Bay*, and along the southern coast of it. The water so displaced, would be followed of course, by the adjacent water *behind* it,

in the open sea; and so on, successively, to a certain extent. This cause, and not the effect of the *Gulf Stream*, extended to the coasts of Europe, as some have supposed, must surely be referred to, as the origin of the Scilly current.

I. The first case, is that of the Earl Cornwallis East India ship. The circumstance occurred on her outward passage: she was well provided with time keepers, as most of the India ships are.

On the 12th March, 1791, between the parallels of 48° and 44°; and at 3° 45′ of longitude, west of Cape Finisterre, (about 53 leagues), this ship experienced an easterly current, equal to 26 marine miles. Her position being directly opposite to the line of the southern coast of the Bay of Biscay, it is a fair conclusion, that the current was occasioned by the cause abovementioned; or as seamen call it, the *indraught* of the Bay: which, it appears, extends to, at least, 53 leagues from the shore. And as the rate, in this place, exceeds one mile *per* hour, it may be supposed, that the effect extends to a still greater distance.

It may here be remarked, that the same ship, in coming out of the *Chops* of the Channel, a few days before, was *set* twenty four miles to the westward, 15 to the northward; in the course of the 24 hours: that is, 28 miles, in a direction of N. W. by W. This may be supposed to be the same stream of current, in its course from the *Bay* towards *Scilly*.

II. The second fact, is that of the *drift of a bottle*, which was thrown out of a Danish ship, (I believe, sent on discovery) since the publication of the former Paper.

The bottle was thrown out, in lat. $44\frac{1}{2}^{\circ}$, lon. 12° west from Greenwich: that is, about 48 miles to the N. E. of the Corn-

wallis's station, at the time that she began to feel the current, on the 11th March. It was taken up by a centinel on duty, near Cape Ortegal; and, as was supposed, at the moment of its driving into the surf. If this was really the fact, the bottle, according to the date of the letter contained in it, must have been carried, at the rate of half a mile *per* hour, in the direction of about E. b. S. $\frac{1}{4}$ S.; the distance was about 64 leagues.

The report of this circumstance was transmitted by the French consul at Corunna, to the Academy of Sciences at Paris.

It may be observed, that the drift of the bottle was much to the *south* of east; whereas, that of the Cornwallis, was east: that is, both pointed towards Cape Ortegal, or its vicinity; as if the main stream of the current, was concentrated there.*

With respect to the velocity of the current, in the present case, all, of course, depends on the time of the arrival of the bottle at the shore. It might have been thrown up long before it was seen, and washed off again, by the tide, or surge of the sea. The *direction*, the most important point, cannot be questioned.

III. The third fact is very simple, and perfectly conclusive. Off Cape Ortegal, at a considerable offing, Admiral Knight found the current, at the rate of one mile *per* hour, setting to the E. S. E.; that is, nearly *along-shore*.

The reader will immediately perceive that these three

* It is observed, that, in the mouth of the Strait of Gibraltar, between C. St. Vincent and C. Cantin, the currents point in all directions, between SE. and NE. towards the entrance of the Strait, which may be considered as the pipe of a funnel.

facts, converge, as it were, to one point: that is, in the proof, that the waters of the Atlantic flow into the Bay of Biscay, along the north coast of Spain.

It would seem that the north-westerly current, by Scilly, did not, at least in many cases, balance the easterly current round Cape Ortegal, and the land of Finisterre.* The loss of His Majesty's frigate, Apollo, with most of her convoy, may surely be attributed to the operation of this current. Captain (afterwards Commissioner) WALLIS, assured me, that after having made, as he supposed, ample allowance for clearing Finisterre, yet, in the night, he had a very narrow escape from shipwreck. Very many others have been brought into the same kind of danger: so that the land of Finisterre, were it not discernible at a considerable distance; and its offing clear of rocks and shallows; and moreover, situated in a finer climate, would prove a kind of Scilly, to mariners.

I have not been able to obtain any proofs, on record, concerning the course of the current, *round* the Bay of Biscay. I formerly collected some information from a French commander respecting it. He said, that the setting of the current along the coast of France, to the north and north-west, was a fact well understood; and even acted on by many, in the choice of the *tack*, on which the current gave the greatest advantage, with dead winds.

One circumstance, and that a very striking one, in respect

* Nor, admitting an equal rate, in both places, could it well be. For the current enters the Bay of Biscay, in an *east* direction, but goes off from it, *north-west*. So that, if a ship was carried fifty miles to the NW. from Ushant, she would only have made about thirty-five westing: but in the other case, she would be carried the *whole* fifty, eastward towards the Bay, and Cape *Finisterre*.

of this particular, is, that the soundings in the Bay of Biscay show little, or no *muddy bottom* to the *southward* of the *Garonne* river; but every where to the *northward*. This seems to show that the mud of the *Garonne*, *Charante*, *Loire*, &c. &c. is all carried to the northward; and by what cause, but a northerly current? Had the motion of the sea been variable, the mud would surely have been distributed, to the south, as well as the north, of the mouth of the Garonne. The alluvial *embouchures* of the rivers in general, here, and the positions of the banks formed by them, in the sea, point to the N. or NW.; apparently the effect of the same sea current.*

IV. In continuation of this current, along the Bay of Biscay, I shall next mention, that Captain (afterwards Admiral) JOHN PAYNE assured me, that being in His Majesty's ship, Russell, in a severe gale of wind at SW., and with the ledge of rocks called the *Saintes*, not far to leeward, he was under apprehensions for the safety of the ship, during a whole night: but to his surprise found himself carried clear of the danger, by a current; which set the ship, in all, about seventy miles to the north-west.

V. The flowing of the tides, on the west of Scilly, cannot well be accounted for, on any other supposition, than that the flood is prolonged by a southerly current. The flood tide is

* From a view of the chart of soundings, between Spain and Ireland, one might be led to suppose, that the deep water and steep shore, along the north coast of Spain, had been *partly* occasioned by the water driven in from the Atlantic, in westerly storms, along that coast; and which had gradually worn away the matter *there*, and deposited it on the bank, which extends from Bayonne to the westward of Ireland. For the bank seems to expand, as it goes northward, in like manner as the current: and the water is shallower than might be expected, in proportion to the depths, farther in.

known to run nine hours to the northward; but the ebb, in the opposite direction, only three hours. This particular had not come to my knowledge, when the Paper of 1798 was written.

VI. But the most satisfactory proof, not only of the *existence* of a *northerly* current, athwart the mouths of the British and Irish channels, but also of its *velocity* (at least during certain intervals), is a statement in a book published in 1785, entitled JOSHUA KELLY's Treatise of Navigation,* (in two volumes octavo). This case is the more satisfactory, as it happened in a *dead calm* of forty-eight hours continuance: so that all uncertainty, regarding the accuracy of a *sea reckoning*, allowances for *leeway*, *drift*; &c. is precluded; since the changes of position that took place, could only have been effected by the motion of the sea, either in the nature of a *current*, or of a *tide*: and this latter must be placed out of the question, since the interval of time, included no less a space than that in which four *fluxes*, and as many *refluxes* have their periods: so that they may well be supposed to balance each other.

"It has been observed (says Mr. KELLY)† by an experi-
"enced commander, who used the West India Voyages for
"many years, from England, (in his return from one of
"these) that in about the latitude of 48° 30', open with our
"British channel, having a good observation (of latitude) at
"the same time, it proving calm and smooth water, insomuch
"that he handed his sails, and so lay forty-eight hours. The

* This was originally pointed out to me by Mr. JOHN PURDY, the very able hydrographer, employed by Messrs. LAWRIE and WHITTLE, Fleet-street.

† Volume the first, page 434.

" first twenty-four hours at noon, he observed the latitude " again, with clear weather; and found by the same that he " had *drove to the northward twenty miles;* which made him " distrust his former observation, though his mate agreed " with him; because the ship had not gone, to his knowledge, " one mile: and upon review, he found that he was not mis- " taken. The next twenty-four hours, being still calm, he " had again another good observation; and then found him- " self about twenty-six miles to the northward of his last " observation; which confirmed him that he was right, the " day before; and that this must be imputed to a strong " northern indraught, or current, there. For when you come " near the soundings, and till you bring Ushant south of you, " on the E.S.E. course,* *you will hardly hold your latitude;* and " the general course is E. N. E. or E. b. N; if but a small " matter to the southward of latitude 49°. And he says, " that would have been my course, if we had not met this " opportunity of discovering this strong indraught: and " for want of observation [i. e. if he had not known the lati- " tude] must have run up St. George's channel, or the north " channel, as many have, and still do, for want of the same " [information.]

" After his last observation, the wind sprang up; and " making allowance for the said indraught [i. e. in his future " course], the next day he was brought into soundings; and " the following day, he was brought in sight of the *Lizard,* " by steering to the southward of the east.†"

* These are *compass* bearings. The magnetic variation, at that time, being about a point and a half, westerly, these will be respectively E ¼ S; NE ¼ E; and ENE ¼ N; true.

† Meaning, no doubt, the ESE course, by compass, as above, or true E ¼ S.

It will naturally occur to the reader, that although this case gives the *northing only;* yet that, in respect of the main question, which is, the danger of shipwreck, on Scilly; or of being carried into the Bristol Channel; it is sufficient to produce a conviction of the necessity of attending closely to the ship's course, when on the point of entering the British channel, after, or during, a course of strong westerly, or south-west winds. But it would, doubtless, have been more satisfactory, had the *direction* of the stream been known. Had that been *north-westerly*, as I have before supposed, the rate of velocity must have been more than a mile and a quarter *per* hour; or approaching to one and a half (the northing being twenty-three at a mean in the twenty-four hours): whilst that in the Atlas East Indiaman, recorded in a former Paper, was about one mile *per* hour, during four days, consecutively.

The statement in Mr. KELLY's book, which is indeed, altogether, more brief than could be wished, is also defective through the want of the distance sailed, from the place of the last observation for the latitude, to that, from whence they saw the Lizard point. They had their first soundings, the day after that observation; and on the following day, they saw the Lizard. His course appears to have been regulated with a view of preserving nearly, his parallel of 49° 16′; to which he had been carried, by the current. It is not likely that he sounded to any great depth: perhaps seventy fathoms; which in that parallel might have been about twenty leagues south-west from Scilly: and it does not appear that he considered himself in soundings, when the calm began; which

however, it is probable he was, although in deep water.* Accordingly, one may conceive that his position, *at the end of the calm*, might have been about the meridian of Cape Clear, or somewhat to the eastward of it. It must be recollected, that in running towards the channel, after the calm, he had still to encounter the same adverse current: and that, possibly, to within thirty or forty miles of his seeing the Lizard.

But, whether his position, during the time that he was under the influence of the current, be a degree more or less, to the eastward, the fact bears the same on the main question; since a ship, in crossing the stream, wheresoever it may be situated, must have been carried out of her reckoning; and thereby placed in danger; in the event of thick weather happening subsequently, and preventing their setting themselves right, by an observation of latitude.

His idea of the eastern edge of the stream, is worth remarking; as it approaches, in a general view, to the truth. It was, that in *about* the parallel of 49°, it approached to the meridian of Ushant. And with respect to the *direction* of the stream, as he calls it a *northern indraught*, he certainly concluded that it ran to the northward, into the St. George's, or Irish channel; brushing the west side of Ushant, and the Land's End. And the effect of the current, on his ship, was no doubt, such as to warrant that belief, with those whose knowledge of the subject was confined to the mere effect of setting them to the northward of Scilly, and into the mouth of the Bristol channel.

The information contained in this statement, does not even

* Perhaps thirty to thirty-five leagues to the west of *Ushant*, and in about 100 fathoms.

terminate in the mere facts of the existence, and force of the current. The commander of the West India ship, is said to have made *many voyages* to, and from, that quarter; and his narrative shows him to have been *an observant man*. Yet *he* was ignorant of the existence of such a current, until the case occurred, which has been just stated. This then, alone, may serve to show, very satisfactorily, that the current does not exist in strength, but at certain intervals: and therefore operates in a more dangerous, because a treacherous manner.

Had it constantly prevailed, like that round the Cape of Good Hope, &c. it could not have escaped detection; and, in consequence, few, or no evils, would have ensued: but these effects being only felt casually, they were considered as mere contingencies, arising from wind and weather, as in other parts of the sea; and not as resulting from a fixed cause, always operating, although in very different degrees: since no person at that time, had collected the different cases, with a view to examine, and to compare them. Some indeed, referred it to the indraught of the Bristol Channel; without considering, that if such a power existed at all, it was difficult to conceive how it could be suspended; and why it should not operate at all times.

Our navigators, in earlier times, appear to have entered the British Channel, on a more southerly parallel, than they have done in latter times. For, although they might have been ignorant of the real cause of the disturbance in their course, yet many of them believed that there was an *indraught*, as they called it, into the St. Georges's Channel: so that one effect of the current; that is, the *northern set*, had not passed unobserved, although the *cause* was not understood: nor, of

course, could it be known, when to expect it. But I have also heard it remarked by sea officers, as long ago as I can remember, that "it was unaccountable, what should occasion "their *running down so much distance*, in coming in with the "land, from the westward." I never heard, however, that there was any suspicion of a current, setting to the westward.

The idea of a northern *indraught* into St. George's Channel, (but which applies equally to the current west of Scilly) is clearly set forth, in a publication by Captain JOSEPH MEAD, in 1757; but which only came to my knowledge very lately, by the favour of Mr. PURDY; to whom I stand indebted, also, for the knowledge of the important fact of the *set* of forty-six miles, during the calm, in the Chops of the Channel.

Captain MEAD first relates the case of the ship Hope of Liverpool, bound from the coast of Guinea, to that port, in November 1735. (Preface, page iii.)

"Having had a good observation, by which they found "they had the Irish Channel open, the wind continued to "blow strong from between the south and west, but mostly "from the former. Having no other observation [of lati-"tude] for six days, in which time, they carried sail, con-"stantly, they by reckoning expected to fall in with Cape "Clear; but in the following night, they fell in with the "*Blasquets*." These islands and rocks are situated in lat. 52° 10′; or about forty-eight miles to the north, and one degree of longitude to the westward of Cape Clear.

Again (page 10) he says, that the Bristol merchant ships, which fall in with Cape Clear, on their homeward passage [from the West Indies, &c.] shape their course from thence, with a large wind, to the high land, near *Padstow*; which is

the land they choose to make, to lead them to the entrance of the Bristol Channel. That in estimating this course, they allowed four or five degrees in the bearing, to compensate for the indraught into St. George's Channel. This angle would give about thirteen or fourteen nautic miles: and is probably what they found by experience, to be the general amount of the *northern set*.*

He goes on to say, that, in like manner, the safety of ships, after they come into soundings, till they reach Scilly, depended on their making *no less allowance*, than the Bristol men do, in the other Channel. For, says he, "experience "informs me, that from the commencement of soundings, in "lat. 49° 30′ N. to the length of Scilly, in *fair weather*, I had "found the northern indraught to be six or eight miles in "the twenty-four hours."

Here then, the fact of the *northern set*, is a second time recognised; though without any suspicion, any more than before, of there being a *westerly set*, also.

Here it may be proper to state, what appears to me to be a very important fact; although perhaps, not connected with the current in question; but materially affecting the safety of the navigation, between the British Channel and Dublin. It was communicated to the Author, by Captain EVANS, a gentleman who superintends the harbour works at Holyhead; and who has had much experience in the navigation of the Irish sea.

* Although they might not have known at that day, the *true* latitude of Cape Clear, yet it may reasonably be supposed that they knew the quantity of the *difference of latitude*, between Cape Clear, and the high land of Padstow; as it was so necessary to their purpose, and so easy to be obtained.

All navigators, says he, in their voyage from the Land's End to Dublin, find themselves, more or less, carried to the eastward, whilst running up St. George's Channel: which is the cause of so many vessels finding themselves in Cardigan Bay; where, in tempestuous weather, and westerly winds, many have been lost. And this he justly supposes to be occasioned by a current setting to the north-eastward.

If the stream, which occasions this disturbance in the reckonings of vessels, here, be a portion of the Scilly current, it cannot well happen in any other way, than by the eastern part of that current falling on the Irish Coast, to the *east* of Cape Clear; and being thence diverted to the north-east, along the south-east coast of Ireland. This may certainly happen; and may form a part of the cause. But I conclude that the principal part of the cause, is, a current *generated* on the south-east coast of Ireland, by the prevalency of south-west and WSW. winds; to which, the position of the coast, between Cape Clear and Carnsore Point, seems particularly adapted.

This effect, from whatsoever cause it may arise, ought to be generally known; as it may produce great inconvenience and distress, to those, who for the first time make use of that navigation: and especially to such vessels, as are either not calculated, or not in a state, to beat off a *lee shore*: for the recesses of the Cardigan Bay are deep, and without shelter.

It may be conceived that a current, so generated, on the south-east coast of Ireland, (and possibly augmented by a portion of the stream from the Bay), would *shoot off* to the north-eastward, pointing towards the Bay of Cardigan; as it

cannot *turn short round* so acute a point, as that formed by the Cape of *Carnsore*: such being the nature of all currents, whether of water, or of air. And vessels will be carried to the north-eastward, accordingly, whilst they continue in the stream of the current. The *southerly* current which passes by Dublin, enters probably into, and merges in the stream in question; as the same kind of current, on the eastern side of England, falls into that, which passes the Strait of Dover, and afterwards runs along the Coasts of Flanders, Holland, &c.

The use of being well informed, concerning these *partial* currents, in narrow seas, is obvious: since the want of such information may, in a moment, be fatal to a ship, and her whole crew. This cannot be more strongly enforced, than by calling to mind the circumstances under which a frigate was lost, with her crew, during the war, just terminated. She sailed either from the Downs, or the Thames, to the *Helder*, and ran in the night, under full sail, on one of the shoals, lying before it. Had the Commander known that there is a general *set*, or current, from the strait of Dover (i. e. at the *back* of the Goodwin Sand), along the Coast of Flanders, Holland, Jutland, &c., and which is estimated to be equal to twenty-five miles, on an ordinary passage to the Texel, he would not have run on, during the night: or, at least, without the precaution of sounding.

Again, the Britannia India ship, in 1809, was lost on the *back* of the Goodwin Sand; probably through ignorance of the *acceleration* of the same current, during a violent gale at west, or south-west. A pilot would be reckoned deficient, who did not know the direction and force of the several streams of tide, at all seasons, within his province. There

is a current generally, if not constantly, running up the British Channel; that is, the eastern tides are the strongest; and in stormy weather from the west, *run longer* than the western, (or ebb) tides. At the same time the level of the channel is raised two feet or more, above that of the North Sea; and consequently, the former will discharge plentifully into the latter. Here then, is an *acceleration* of the current; and which men who have the charge of piloting of ships, ought to have known. And who can be ignorant of the high level of the Channel, when they know that the ports in the Channel are some feet deeper in strong westerly winds, than at ordinary times?

Although the following remarks do not apply to the *Scilly*, or *Thwart* current, yet as relating to currents, that at all times affect the navigation around the British Islands; it is hoped that the utility of inserting them, may atone for their being out of place.

It is proper to state, that the facts here set forth, are assumed, on the ground of *detached* notices, and not from a connected chain of observation. Indeed it could hardly have been expected. And it is also proper to be stated, that the currents here intended, only form a portion of the ordinary stream of tide, along the coast. And it is the *difference* between the degrees of velocity of the opposite streams, on the same coast, that constitutes the current in question: as indeed, it cannot be manifested, in any other way. An instance has just been given in the British Channel.

Where rivers form any quantity of alluvial ground, at their entrance into the sea, there, most infallibly, the direction of the

sea current will be shown, by the arrangement of the alluvial ground; or by the sand or mud banks contiguous to it, in the sea. The process is explained in a note: and those, to whom I may not have rendered myself intelligible, may easily satisfy themselves, by observing the junction of any two streams, that are very much disproportioned to each other, in point of bulk; and in which, no art has been employed to counteract the natural course of things.*

* The point of junction of two rivers (or of a river with the sea, provided that the sea has a predominant stream of current) will always form an *acute angle*, if the soil, through which they run, be not of a texture, firm enough to resist the corroding power of the stream; but composed of alluvial matter, deposited by one, or both of the waters; as is ordinarily the case. This point of junction, may be either *firm alluvial land*, or a *bank* of *sand or mud*, under water; as the case may happen. And finally, the acute angle of junction will always point in the direction of the stream of the *recipient* water; be it a river, or the sea.

The reason of this is, that two streams, at their confluence, have a natural tendency to *slide* into each other, as the easiest mode of effecting their junction; and were they, either by reason of the natural solidity of their banks, or by artificial means, compelled to join at right angles, or at a very large angle, the meeting of their waters, in a case where they had any degree of rapidity, would produce an agitation; that would prove injurious to their banks, and inconvenient to the navigation.

For the sake of illustration, let it be supposed that a small river is conducted artificially, into a larger one (or into a sea which has a current along shore,) through a *cut* made through the alluvial soil; and the angle of junction to be very large, or approaching to a right angle; and without any artificial aid, to keep it in that state; the following train of consequences would ensue: (It is to be supposed, of course, that the recipient river had its bed previously enlarged, to receive the other, in order to prevent floods).

The first effect would be, that the head of the *adjunct* river, entering with an almost perpendicular course, into that of the *recipient*, would meet with so much resistance from it, that it would be partly beaten back, and compelled to seek its way along the bank of the recipient river. This bend in its course, would induce such a pressure on the bank, at the *lower* angle of junction, as would soon wear it away; and an *oblique course* of approach, of the whole body of the adjunct stream,

In effect, the *embouchures* of rivers, situated in alluvial ground, always *point in the direction of the stream of the sea current*. The sand, mud, or gravel banks formed by the same current, *lie in the same direction;* and with their *narrowest*, or *sharpest* point, downwards.

In many places along the coast, water courses are found to terminate in long narrow lakes, with narrow sandy tracts, between them and the sea (through which the water still oozes, though all appears to be *stagnant*). These also have been formed by the sea currents, and point the same way. Originally, these water-courses, or rivers, gained the sea, near the place where the *head* of the lake now is; but the sea current, forming sandy alluvion, along that part of the coast, the river kept its course within it, along the former sea shore;

would commence. In the mean time, *a triangular space* of *still water*, would be formed between the *original* upper angle of junction, and the *new* one, occasioned by the obliquity of course: and this *still water*, as is its nature, would let drop the mud and sand which it had held suspended, whilst in motion; and thus begin to form *a triangular bank*, of the same shape and extent,*

Here then the operation is commenced, in all its parts: and the *triangular bank* by its being constantly on the increase, will force the adjunct stream to borrow still more and more, on its opposite bank; which will gradually wear away, until the *angle of junction* of the two waters, becomes so acute, that the adjunct stream no longer meets with any resistance, from that of the recipient; but may be said to *slide* into *contact* with it. This then, is the *natural state* of the junction of streams: but after all, the point of junction will, although almost imperceptibly, move downwards; because the *triangular bank* must continue to receive additions, if left to itself. Mean time, the body of it rises by the continual depositions, above the surface of the water, and becomes firm alluvial land; its *apex* being the point of junction of the two waters; and its *direction*, of course, the same with that of the stream of the recipient water.

When two streams that are nearly equal to each other, in respect of bulk, and of velocity, join; each of them, as it were, asserts *its own rights*; and the collective

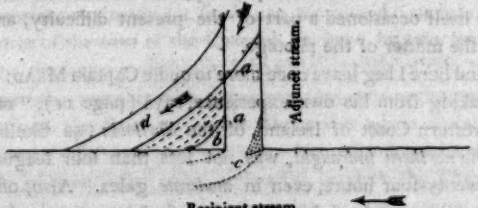
* The reader is referred, for explanation, to the sketch on the following page.

but growing sluggish, for want of *declivity*, it formed a lake within the increasing alluvion: and occasionally, in time of floods, forced open again, a communication with the sea. These lakes equally prove a sea current, as well as its line of direction.

If the reader requires examples, on a large scale, the mouths of the *Senegal* and *Mississippi* rivers, may be referred to. In both cases the currents are well known. And for lesser ones

stream takes a direction, which is generally a mean, between those of the two streams, whilst they remained in a separate state. And in like manner, the falling in of a stream that bears any proportion to the recipient river, will occasion a proportionate determination of the collective stream, towards the line of the former course of the adjunct.

It will be recollected, that all that was meant to be said here, applies to the courses of streams, through alluvial ground: and also to such, as have some degree of velocity.



(*a*) Artificial cut, through alluvial ground.

(*b*) The angle of the bank first worn away.

(*c*) The commencement of a triangular bank formed by the *still water* above the point of junction.

(*d*) Progress of the adjunct stream, towards a more permanent junction.

the rivers on the coasts of our own Islands; as those of *Christ-church*, *Shoreham*, *Newhaven*, *Orford*, *Yarmouth*, *Dublin*, &c. The small water-courses prove, equally with the large ones, the state of the current.

It is assumed, on the ground of various notices that the author has collected, that along the western side of Ireland, occasioned by the prevalence of westerly winds, the preponderance of the stream is northwards:* that it turns round the north end of the Island (or at least, a part of it does,) and thence southward, along the eastern coast: probably the whole way to Carnsore Point; but certainly past Dublin. The effect of a southerly current, or prevailing southerly tide, over the northern, is visible in the present *outfall* of the Liffey; and still more in the traces of the former one; previous to the erection of the *long wall:* both having an inclination to the south. And possibly, the *direction* of the wall has itself occasioned a part of the present difficulty, arising on the matter of the pilotage.

And here I beg leave once more to quote Captain MEAD; who, speaking from his own experience, says (page 11), "on the "western Coast of Ireland, off the *Shellocks* (qu. Skelligs?) "the *northern indraught*, was not less than four leagues in "twenty-four hours, even in *moderate* gales. Also, off the "western coast of *Lewis* Island, I find it stronger than in "*soundings*, [that is, more than six or eight miles in the "twenty-four hours]; and also, off *Foul* Island, (Shetland) "something weaker than the latter."

Along the south-eastern coast, from the Mizen head, and

* Articles of various kinds, known to have come from the southward, and south-east, are continually casting up in Galway Bay.

Cape Clear, to Carnsore Point, the Author is less informed, in respect of notices from others, than concerning any other part of the Coast; but having visited it repeatedly, and considered all the circumstances belonging to it, he is firmly persuaded that there is a *north-easterly* current; and that it is *this* stream, prolonged from Carnsore Point, that carries vessels to the eastward of their course, in their way up the Irish Sea. (See above, page 194.)

The same kind of northerly stream, and occasioned, probably, by the same cause, is produced on the western coast of Scotland; from whence it turns round the north end of the island, and thence southward, along its eastern side, as far as Harwich; where it falls into the strait of Dover, or Channel Current, which comes up at the back of the Goodwin Sand.

The Channel Current has already been mentioned (page 196.) and can hardly be questioned, as to its existence, when the circumstances already set forth, are considered: such as the *elevation* of the *level* of the Channel, at times, by two feet or more; the *longer continuance* of the *eastern* stream of tide, than the *western*; together with the stream that runs to the north-east, from the Strait of Dover, along the whole Coast of Flanders, Holland, and Jutland: and which, affording, as is said, a help of twenty-five miles, ordinarily, between the Thames and the Texel, a run of only 160 miles, or less; cannot but be referred in part, to the Channel, or Dover Current. (For more particulars concerning the current at the Strait of Dover, &c. the reader is referred to Vol. XVIIth. New Series, of the Society's Transactions.)

At the mouth of the Baltic sea, the Jutland current is joined by the *outfall* of the former; which, at all times, receives more

water than it can evaporate. The collective stream then proceeds along the coast of Norway; continuing its northerly course. Off the *Naze*, it is known to run two miles *per* hour.

Such is the general course of the stream of current, around these Islands. There are, however, many particular exceptions to be made; as happens in the case of tides, where a deep recess in the coast often disturbs the uniformity of the general course of the stream.

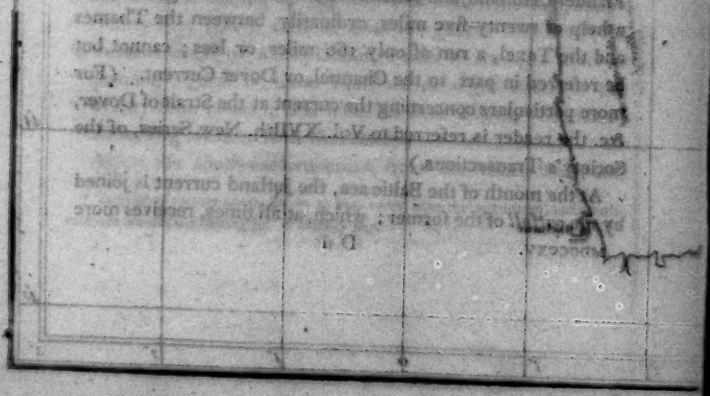
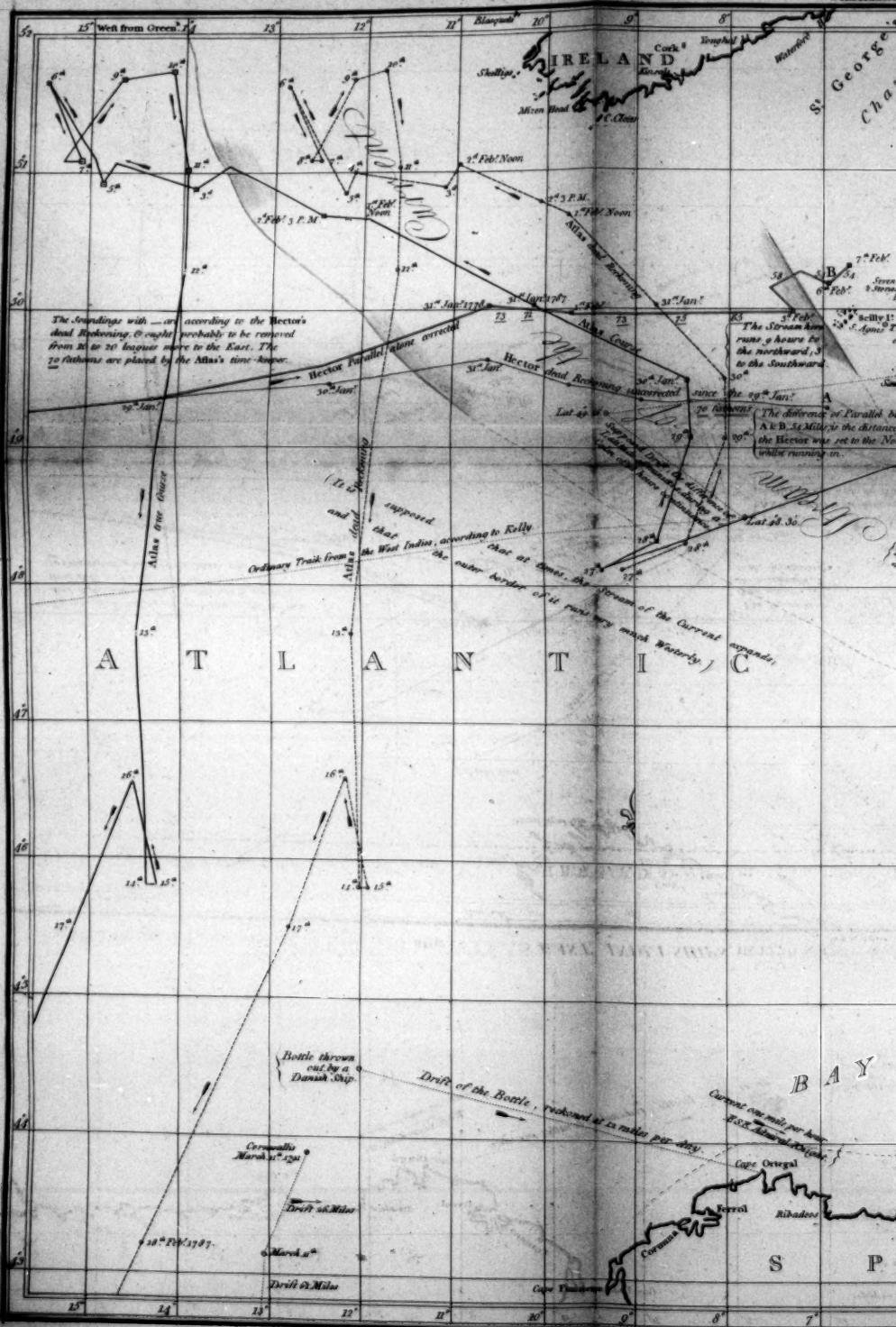


CHART of the *TRACKS* of the HECTOR and ATLAS *EAST INDIA SHIPS*, in 1778 & 1787, Exh

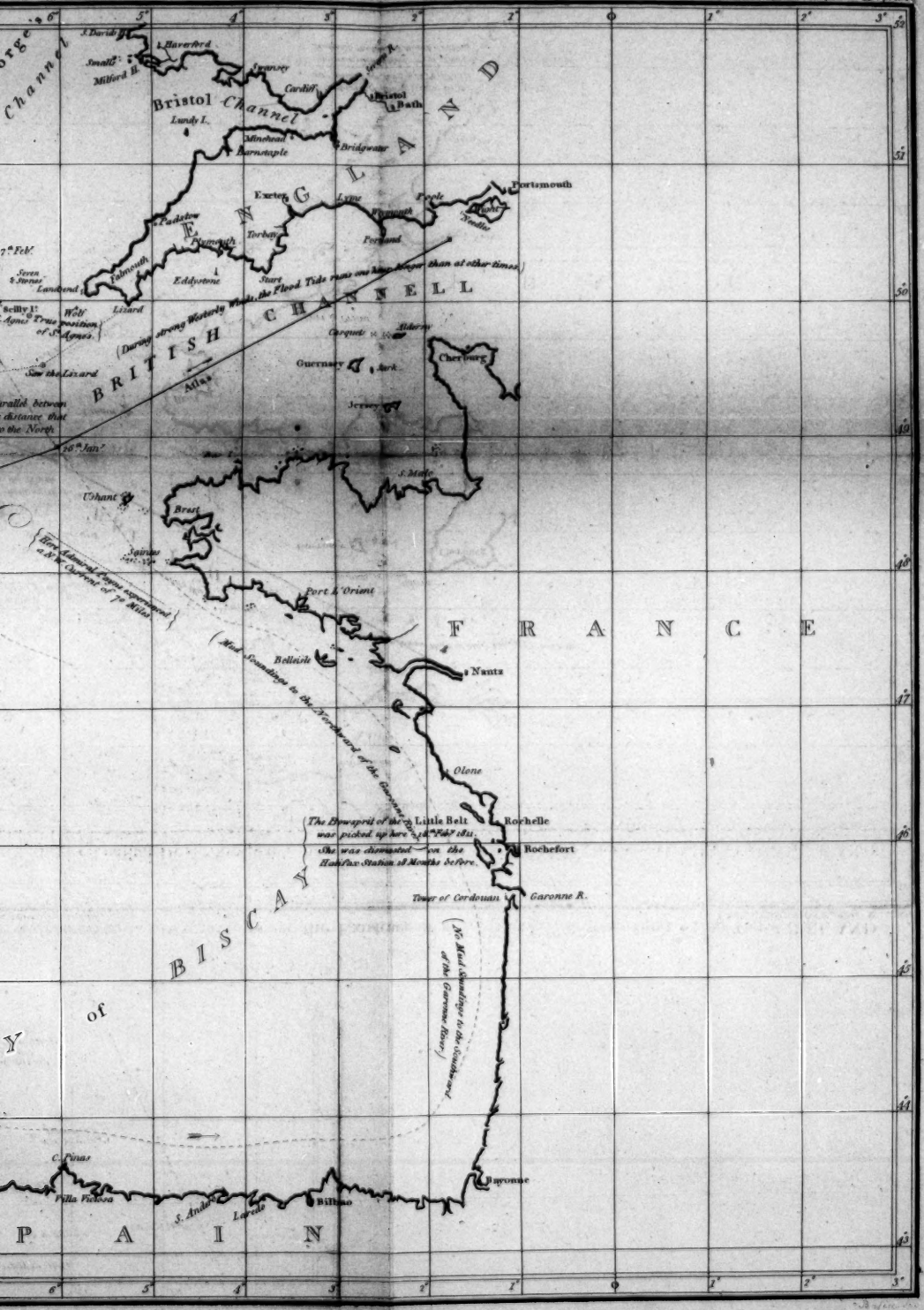
With Additio


West from Greenw.
IRELAND
Cork
Youghal
Waterford
Shallipe
Mizen Head
C. Clear
St. George
Chan
Hector
Atlas
Hector dead Reckoning
Atlas dead Reckoning
Hector Parallel Atlas corrected
Hector dead Reckoning uncorrected
Atlas Course
Atlas one Course
Atlas dead Reckoning
The Soundings with — are according to the Hector's dead Reckoning, & ought probably to be removed from 30 to 40 leagues more to the East. The 70 fathoms are placed by the Atlas's time-keeper.
The Stream has run 9 hours to the Northward, 3 to the Southward.
The distance of Parallel A & B, 34 Miles, is the distance the Hector was set to the No. whilst running in.
Lat 49 36
Lat 48 36
Ordinary Track from the West Indies, according to Kelly
and that supposed
that at times, the outer border of it runs very much Westerly
the Stream of the Current expands
ATLANTIC
Bottle thrown out by a Danish Ship
Drift of the Bottle, reckoned at 12 miles per day
Current one mile per hour
N.N.W. Mineral Waters
BAY
Cape Ortegal
Ferrol
Ribadeo
Corunna
Cape Finisterre
Cornwallis
March 11th 1781
Drift 18 Miles
March 11th
Drift 6 Miles
18th Feb.y 1787
S P

, Exhibited with a Design to prove the Existence of a CURRENT, between USHANT and IRELAND.

Additions in 1816.

Philos. Trans. MDCCCXV. Plate X. p. 309.



XII. *Some experiments on a solid compound of iodine and oxygene, and on its chemical agencies.* By Sir HUMPHRY DAVY, *LL.D. F.R.S.*

Read April 20, 1815.

IN the two papers containing researches on iodine which the Royal Society has done me the honour of publishing in the Transactions, I have described a class of bodies consisting of iodine, oxygene, and different bases analogous to the hyper-oxymuriates. In the last of these papers, I mentioned, that I had not been able to procure any binary combination of iodine and oxygene from these compounds, neither by the method proposed by M. GAY LUSSAC, namely, the action of sulphuric acid on the oxyiode of barium, nor by other methods of my own institution; and that in experiments on the effects of the acids on the oxyiodes, new combinations only were formed. I have lately resumed this enquiry, and by pursuing a new and entirely different plan of operation, I have at last succeeded in combining oxygene and iodine. In the following pages I shall describe the circumstances which led me to ascertain the existence of this compound, and I shall detail some experiments on its analysis and its chemical agencies.

In the course of my researches, I observed, that when a solution of the compound of iodine and chlorine was poured into alkaline solutions, or even into certain muriatic solutions, the precipitate was an oxyiode; and this fact seemed to indicate, that iodine had a stronger attraction for oxygene than

chlorine; iodine, likewise, has an attraction for chlorine; it appeared, therefore, extremely probable, that euchlorine, or the gaseous combination of oxygene and chlorine, would be decomposed by heat, and two compounds formed, one of oxygene and iodine, and the other of iodine and chlorine, or that a triple compound would be produced from which chlorine could be easily separated, and on submitting the idea to the test of experiment, I found that I had not been deceived.

To produce the compound of oxygene and iodine, it is necessary merely to bring the euchlorine and iodine together at the ordinary temperature of the atmosphere. As soon as the euchlorine comes in contact with the iodine, there is an immediate action, its colour changes to bright orange, and a liquid is formed. When the euchlorine is in sufficient quantity, a white substance likewise appears. By the application of a gentle heat, the orange compound of chlorine and iodine may be made to rise in vapour; and the compound of oxygene and iodine remains.

When this compound is required to be dry, the euchlorine should be passed through dry muriate of lime (calcane) before it is admitted to the iodine. The apparatus that I have employed for producing the substance is a curved bent tube, in the form of an inverted L (⅂), closed at one end, the closed leg of the tube being longest, and which serves as a retort for generating the gas; a thin long-necked glass receiver for containing the iodine, and a curved tube of smaller diameter than the first, and cemented or ground into it for conveying the gas into the receiver. The muriate of lime is placed in some dry paper in the upper part of the large curved tube;

and to produce the substance from 40 grains of iodine, 100 grains of the hyperoxymuriate should be used, and four times the quantity of solution of muriatic acid of specific gravity about 1.105; a very small spirit lamp should be employed to generate the gas; and to prevent explosions, the heat should be applied with great care, and only to the bottom of the tube.

The compound of oxygene and iodine when entirely freed by heat from the compound of oxygene and chlorine, appears as a white semi-transparent solid; it has no smell, but a strong astringent sour taste. Its specific gravity is considerable, for it rapidly sinks in sulphuric acid. When heated strongly, it decomposes, undergoing fusion at the moment, and is entirely converted into gaseous matter and iodine, leaving no residuum whatever.

It requires for its entire decomposition, a heat which is rather below the boiling point of olive oil, and there seems to be little or no increase of temperature in the process.

Its nature is proved both by analysis and synthesis, for when euchlorine acts upon iodine, the volatile substance produced, has all the characters of the body produced by the immediate action of chlorine on iodine; and when the compound I am describing is decomposed in a pneumatic apparatus, the gas formed is found to be pure oxygene, and the solid sublimate produced, is pure iodine.

I endeavoured to determine the proportions of the elements in the compound, by decomposing it in glass tubes carefully weighed, and ascertaining the loss of weight of the tube, and the volume of oxygene evolved. I have used very small quantities of the substance, but as my balance is delicate, I

do not think there can be any considerable error in the results. I give those which I consider as the most accurate.

In one experiment, 3 grains of the substance afforded a quantity of oxygene equal to 517.3 grain measures of water, and lost in weight .68. In a second experiment, 2 grains afforded 348.3 grain measures of oxygene. In a third experiment, 1 grain yielded 191 grain measures of oxygene.

Many experiments that I have lately made, have convinced me, that in my first paper I rated the number representing the proportion in which iodine combines too low; indeed at the time, I stated that my results afforded rude approximations, they demonstrated merely that iodine was represented by a very high number. In an experiment recently made with care, 50 grains of the iode of potassium decomposed by nitric acid, afforded 32.8 grains of nitre. According to this result, the number representing the proportion in which iodine combines is 227.3; but I do not venture to state this number as exact, as I am not secure of the purity of the hydrate of potassa from which the iode was made.

The compound of iodine and oxygene is very soluble in water; it slowly deliquesces in a moist atmosphere, but remains unaltered when the air is dry; its solution first reddens, and then destroys vegetable blues, and reduces other vegetable colours to a dull yellow. When its aqueous solution is heated, as the water rises in vapour, it gradually thickens, gains the consistence of a syrup, becomes pasty, and at length by a stronger heat yields the solid substance unaltered; unless a sufficient heat is applied to decompose a portion of it, when it gains a purplish tint apparently from some iodine set free. The pasty substance that it forms is evidently an hydrate, for it yields moisture during its decomposition.

Its action upon inflammable bodies is such as might be expected from its composition. When it is heated, mixed with charcoal, sulphur, resin, sugar, or the combustible metals in a finely divided state, detonations are produced; and its solution rapidly corrodes all the metals which I have exposed it to, and it acts both upon gold and platinum, but much more intensely on the first of these metals.

When a solution of it is poured into solutions of the alkalies or alkaline earths, or when made to act on their carbonates, oxyiodes or triple compounds of oxygene, iodine, and the metallic bases, are the results. By its action on solution of ammonia, a substance is produced apparently the same as that which is formed by the action of the compound of iodine and chlorine, saturated with chlorine on the same solution, and which I have mentioned in a former Paper, and which, consequently, must be regarded as an oxyiode of ammonia.

When an aqueous solution of the compound is poured into a solution of the soluble salts of baryta and strontia, a copious precipitate of their respective oxyiodes is produced. The oxyiode of barium, as I have mentioned in my last communication on iodine, is a compound very slightly soluble in water, that of strontium is rather more soluble; and those of calcium, magnesium, glucinum, ittrium, aluminum, zirconum, are more so, and I believe in the order in which they have been named.

It forms combinations with all the metallic oxides I have tried its agency upon, and precipitates lead and mercury from their nitrous solutions.

The action of the compound upon acids is much more singular than that upon alkalies, earths, or metallic oxides. It

appears to form combinations with all the fluid or solid acids to which I have been able to expose it, that it does not decompose. When sulphuric acid is dropped into a concentrated solution of it in hot water, a solid substance is precipitated, which consists of the acid and the compound; for on evaporating the solution by a gentle heat, nothing rises but water. On increasing the heat in an experiment of this kind, the solid substance formed, fused; and on cooling the mixture, rhomboidal crystals formed of a pale yellow colour, which were very fusible, and which did not change at the heat at which the compound of oxygene and iodine decomposes, but sublimed unaltered. When urged by a much stronger heat, it partially sublimed, and partially decomposed, affording oxygene, iodine, and sulphuric acid.

With hydro-phosphoric acid, the compound presents phenomena precisely similar, and they form together a solid, yellow, crystalline combination.

It dissolves in solution of hydro-phosphorous acid, but on heating the combination, iodine is immediately produced by a decomposition of part of the compound, and the remaining part unites to the phosphoric acid formed.

When hydro-nitric acid is poured into a concentrated solution of it, white crystals form in plates of a rhomboidal figure, and which when dried, partially decompose, and partially sublime at a much lower heat than the sulphuric or phosphoric compounds, and afford hydro-nitric acid, oxygene, and iodine.

It dissolves in solution of oxalic acid, but by a very gentle heat, the oxygene of the compound acts on the inflammable bases of the acids, and iodine and carbonic acid are disengaged in great quantities.

By liquid muriatic acid, the substance is immediately decomposed, and the compound of chlorine and iodine is formed.

When boracic acid was added to a solution of the new compound, it dissolved in it by heat, and did not crystallize on cooling. By evaporation, a solid white substance was procured, not so easily decomposed by heat as the compound itself.

The taste of all these acid compounds is very sour, though in different degrees of intensity: they redden vegetable blues, and they dissolve gold and platinum. When they are made to act on the alkalies or earths, or on saline solutions which they are capable of decomposing, common neutral salts and oxyiodes are formed at the same time.

The facts of the combination of the new compound with acids, serve to explain the phenomena of the action of these substances, on the oxyiodes which I have described in my last paper on iodine, and they confirm the opinions there stated on the nature of this action. The substance procured by M. Gay Lussac, by the action of sulphuric acid on the solution of the oxyiode of barium, and which he has supposed to be a pure combination of oxygene and iodine mixed with a little sulphuric acid, has evidently for its base the combination just now described of sulphuric acid and the new compound, and, as I have shown, it likewise contains baryta. However minute the quantity of sulphuric acid made to act on oxyiode of barium, a part of it is always employed to form the compound acid; and the residual fluid contains both the compound acid, and a certain quantity of the original salt.

That this compound acid is a true chemical combination, is evident from the observations already detailed, and from

its crystalline form. There is every reason to believe, that the proportions of its elements are definite. In one experiment I found, that a small quantity of the new compound in being converted into the rhomboidal crystals, gained rather less than half its original weight from the addition of the acid, i. e. 2 grains became 2.8 grains.

In experiments in which the products of the decomposition of the compounds from phosphoric and sulphuric acids were collected, the acids disengaged were found in their state of hydrates, from which it is probable, that the crystalline compounds are hydrates, and that the common acids carry their definite proportion of water into the combination. It is not indeed unlikely, that the presence of water is connected with the phenomenon of combination, and there is an instance of this kind which I long ago pointed out. Sulphurous acid gas, and nitrous acid gas, appear to have no action on each other, unless water be present; but with the vapour of water they form a solid crystalline hydrate.

Reasoning from analogy, it is probable, that a compound of oxygene may be formed, containing less oxygene than the new compound. I have made many experiments with the hope of discovering a body of this kind; but without any decided success. When the solution of the new compound is made to act on the double compound of iodine and the alkaline metals, iodine is produced, which during its sublimation, yields no gaseous product. Iodine heated in a solution of the new compound slightly colours it, but this appears to be merely in consequence of its combining with the water; and the iodine rises in vapour with the water without decomposing the compound. In some experiments on the action of

euchlorine on iodine, in which the iodine was in great excess, the solid substance formed had a chocolate tint; but this may possibly have depended upon a small quantity of free iodine, and when dissolved in water, it afforded by the evaporation of the water, the white compound only.

I detailed in my last paper on iodine, some unsuccessful attempts to procure a compound of oxygene and iodine from the chlorionic acid, the substance produced by the agency of the combination of iodine and chlorine in water, on the idea that water was decomposed in this experiment. I have made some further researches, on the supposition that it might contain a compound of iodine containing less oxygene than this new substance; but without any success: neither by distillation at very low temperatures, nor by the action of small quantities of oxide of silver, nor by any other means, have I been able to separate any compound of oxygene from it; and when it forms triple compounds, the oxyiodes, by its action upon alkalies, or earths, or metallic solutions, it appears that the oxygene of the alkalies or earths is only newly combined at the moment of its operation upon them, an effect assisted by the attraction of the bases of the earths for chlorine. The conclusion which I formed, that the chlorionic acid is a simple combination of the chlorionic sublimate in water, is still farther proved by the circumstances of the action of muriatic acid on the new solid compound of oxygene and iodine.* Page 209.

* The chlorionic acid offers an easy method of procuring pure baryta. By dropping a solution of it into solution of muriate of barium, as I have shown in my last paper on iodine, a precipitate of oxyiode of barium is produced, which when decomposed by a strong heat, yields pure baryta, the attraction of oxygene for barium being, as I have stated, stronger at this temperature than that of iodine.

As I have called the compounds of oxygene, iodine, and bases, *oxyiodes*, I venture to propose a name in conformity, that of *oxyiodine* for the new solid compound, and *oxyiodic* acid, for the acid compound it forms with water. M. GAY LUSSAC, as I am informed, has proposed in a paper which I have not yet been so fortunate as to procure, but which is said to contain many new and important facts, the name of iodic acid for the compound of oxygene and iodine, the existence of which he conceived he had proved by his experiment on the action of sulphuric acid on the oxyiode of barium, and the terms iodats for the substances, consisting of oxygene, iodine, and bases. I am willing to pay every compliment to the sagacity of this ingenious chemist, in anticipating the knowledge of the nature of a body, the separate existence of which he had not demonstrated by experiment; but the term *iodic* acid does not appear to me sufficiently definite. For the hydroionic acid, and the chlorionic acid, as well as the oxyiodic acid, may be all called as a class iodic acids, or acids from iodine, and the termination in *at*, would place the oxyiodes in the common class of neutral salts, from which they differ in many respects. When they become binary compounds in consequence of their decomposition by heat, though they lose all their oxygene, their neutral and saline character remains unaltered, which is not the case with any other known class of bodies, except the hyperoxymuriates; and the terms iodes and oxyiodes which I proposed in the first paper, in which the distinction between these bodies was pointed out, sufficiently express the nature of the double and triple compound, and the difference between them.

I am desirous of marking the *acid* character of oxyiodine

combined with water, without applying the name acid to the anhydrous solid. It is not at all improbable that the action of the hydrogene in the combined water is connected with the acid properties of the compound; for this acid may be regarded as a triple combination of iodine, hydrogene, and oxygene, an oxyiode of hydrogene, and it is possible that the hydrogene may act the same part in giving character, that potassium, sodium, or the metallic bases perform in the oxyiodes; and as hydrogene combined with iodine forms a very strong acid, and as this acid would remain, supposing all the oxygene to be taken away from the oxyiodic acid, it is a fair supposition that its elements must have an influence in producing the acidity of the substance.

Rome, February 10, 1815.

XIII. *On the action of acids on the salts usually called hyperoxymuriates, and on the gases produced from them.* By Sir Humphry Davy, *LL.D. F. R. S.*

Read May 4, 1815.

The effects produced when concentrated hydro-sulphuric acid (oil of vitriol) is poured upon hyperoxymuriate of potassa, have been often objects of chemical discussion; the acid and the salt, it is well known, become deep orange, and if any moisture is present, or if heat is applied to the mixture, a detonation occurs. In a paper read before the Royal Society, I have ventured to suppose, that these phenomena depend upon the developement and sudden decomposition of the compound of chlorine and oxygene, which I have named euchlorine.

A statement, which I understand has been made by M. Gay Lussac, namely, that a peculiar acid, which he has called chloric acid, may be procured from the hyperoxymuriate of baryta by sulphuric acid, led me to examine the action of acids on the hyperoxymuriates under new circumstances, and I have made some observations which appear to me not unworthy of being communicated to the Royal Society.

If 30 or 40 parts of sulphuric acid be poured upon one part of dry hyperoxymuriate of potassa in a wine glass, and the salt be agitated in the acid, there is a very slight effervescence only, the acid becomes of a deep orange tint, and white

fumes, mixed with orange fumes, fill the upper part of the glass, which have a very peculiar and not a disagreeable smell.

The *slight* effervescence taking place in this process, induced me to suppose that the substance which coloured the acid must contain a larger proportion of oxygene than euchlorine, for I have shown, in a work published in 1819,* that hyperoxymuriate of potassa contains six proportions of oxygene; and by its decomposition 2.5 volumes of oxygene ought to be evolved for every volume of chlorine; and euchlorine procured from the hyperoxymuriate of potassa by solution of muriatic acid, yields only one volume of oxygene, and two volumes of chlorine.

I endeavoured to procure the substance which coloured the sulphuric acid during its action upon hyperoxymuriate of potassa, and after several failures, in which explosions took place, I at length succeeded in the following manner. Dry oxymuriate of potassa in powder was mixed with a small quantity of sulphuric acid, and they were rubbed together with a spatula of platinum till they had incorporated, and formed a solid mass of a bright orange colour. This mass was introduced into a small retort of glass, and exposed to the heat of water which was gradually warmed; a bright yellowish green, elastic fluid arose from the mixture, which was rapidly absorbed by water, giving to it its own tint, but which had no sensible action on mercury.

To make this experiment without danger, not more than 50 or 60 grains of the hyperoxymuriate should be employed, great care should be taken to prevent any combustible matter from being present, and the water should not be permitted to

* Elem. of Chem. Phil.

attain a temperature equal to 212°, which may be easily managed by mixing it with alcohol. There are dense white fumes when the mixture is first made, but there seems to be no heat produced; a small quantity of the orange gas is disengaged at this time; but the greater part of it remains attached to the sulphuric acid in the solid mass, and is expelled from it by the heat.

The gas procured by this process over mercury, when compared with the gas procured from the hyperoxymuriate, by liquid muriatic acid, is found to have a much more brilliant colour, is much more rapidly absorbed by water, has a peculiar and much more aromatic smell, unmixed with any smell of chlorine. It destroys moist vegetable blues without previously reddening them. When it is heated to a temperature about that of boiling water, it explodes with more violence than euchlorine, and greater expansion of volume, producing much light. After the explosion over mercury, rather less than three (from 2.7 to 2.9.) volumes appear for two of the gas decomposed, and of these, two are oxygene, and the remainder chlorine.

A little chlorine is always absorbed by the mercury during the explosion of the gas; and it appears reasonable to conclude, that the deep yellow gas is in reality composed of two in volume of oxygene, and one of chlorine, condensed into the space of two volumes, and that it consists in weight, of one proportion of chlorine 67, and four of oxygene 60.

None of the combustible bodies which I have tried, decompose this gas at common temperatures, except phosphorus; this when introduced into it, occasions an explosion, and burns in the liberated gases with great brilliancy.

Its saturated solution in water is of a deep yellow colour,

it does not taste sour, but is extremely astringent and corroding; when applied to the tongue, it leaves for a long while a very disagreeable sensation.

It occurred to me that the gas procured from the hyper-oxymuriate by the action of liquid muriatic acid, might be a mere mixture of this gas and chlorine; and two in volume of this gas and three in volume of chlorine, would produce by explosion the same products as euchlorine. The only fact which I am acquainted with, opposed to the idea, is the circumstance of dutch foil not burning spontaneously in the gas from muriatic acid, which might be expected if it contained as much as $\frac{1}{4}$ of uncombined chlorine; though the force of this argument is suspended, till it is supported by an experiment showing that dutch foil inflames in a mixture of two of the deep yellow gas, and three of chlorine. I have not yet been able to procure at Rome, metallic foil fitted for this experiment.

I have ascertained that the gas from hyper-oxymuriate and muriatic acid, though it acts much more slowly upon water than the other gas, yet in the end gives it the same tint and properties; and when much of it is exposed to a small quantity of water, it always leaves a residuum of chlorine, so that if it be not a mixture, but a compound, the new gas is formed from it by the action of water.

The action of hydro-nitric acid on the hyper-oxymuriate, affords the same gas as that produced by the action of sulphuric acid, and a much larger quantity of nitric acid may be safely made to act on the salt; but as the gas must be procured by solution of the salt, it is always mixed with about $\frac{1}{5}$ of oxygene. From the solid mixture made with sulphuric acid, I have obtained a gas containing only $\frac{1}{10}$ of oxygene; the fifth

proportion obtained in the experiments with nitric acid, being evolved during the time the mixtures were made.

The saturated solution of the gas affords white fumes, similar to those produced at the moment the hydro-sulphuric mixture is made, from which it is probable, that these fumes consist of a hydrate of the gas.

The saturated solution, when mixed with solution of fixed alkalies, or of ammonia, does not immediately lose its colour, nor neutralise the alkalies; but after some time the effect is produced, and hyper-oxymuriates are obtained, (probably mixed with a minute quantity of muriates). The solution exposed to air, or suffered to remain in close vessels, becomes soon colourless; and I am inclined to believe that this depends upon a decomposition of water, for some of it exposed to a small quantity of air rather increased its volume.

I shall not propose to give any name to this substance, till it is determined whether euchlorine is a mixture or a definite compound, and I hope soon to have the means of making a decisive experiment on this subject.

It appears that this new substance, though it contains four proportions of oxygene, is not an acid; and hence it is probable, that the acid fluid compound of oxygene, chlorine, and water, which M. GAY LUSSAC calls chloric acid, owes its acid powers to combined hydrogene, and that it is analogous to the other hyper-oxymuriates, which are triple compounds of inflammable bases, chlorine, and oxygene, in which the base and the chlorine determine the character of the compound. Muriate of potassa, (potassane) is a perfectly neutral body; and when six proportions of oxygene are added to it, it still remains neutral. Muriatic acid (chlorine and hydro-

gene) is a strong acid; and according to the relation above stated, it ought not to lose its acid powers by the addition of six proportions of oxygene. Till a pure combination of chlorine and oxygene is obtained, possessed of acid properties, we have no right to say that chlorine is capable of being acidified by oxygene, and that an acid compound exists in the hyper-oxymuriates. We know that chlorine is capable of being converted into an acid by hydrogene, and, as I mentioned in my last paper, where this principle exists its energies ought not to be overlooked; and all the new facts confirm an opinion which I have more than once before submitted to the consideration of the Society, namely, that acidity does not depend upon any *peculiar* elementary substance, but upon *peculiar combinations* of various substances.

Rome, Feb. 15, 1815.

NOTE.

SINCE my return to England, I have made some farther investigations on oxyiodine, on the oxyiodes, and on the deep yellow gas. The portable apparatus which I employed in Italy, enabled me to operate only on very minute quantities of oxyiodine; I have lately made my experiments on a larger scale.

Thirteen grains of oxyiodine decomposed by heat, afforded 9.25 cubical inches of oxygene: and 48 grains of oxypotassame or oxyiode of potassium, yielded when decomposed by heat, 31 cubical inches of oxygene gas: and 36 grains of potassame or iode of potassium (a portion of the salt so decomposed,) afforded by treatment with nitric acid 17.8 grains of dry nitre. These results give the number 246 as the number representing iodine, and prove that oxyiodine consists of one proportion of iodine and five of oxygene; and that the oxyiodes contain six proportions of oxygene.

The deep yellow gas when mixed with chlorine in the proportion of 2 to 3, or even of 2 to 2, deprives it of the power of acting upon Dutch foil, though 1 of chlorine when mixed with 2 of common air, still burns this substance. Hence it appears probable, that the deep coloured gas and chlorine have a chemical action on each other, and that euchlorine is not a simple mixture of them. I hope soon to be able to present to the Society, some new results on this subject.

London, June 12, 1815.

XIV. *Farther analytical experiments relative to the constitution of the prussic, of the ferruretted chyazic, and of the sulphuretted chyazic acids; and to that of their salts; together with the application of the atomic theory to the analyses of those bodies.* By Robert Porrett, *jun. Esq.* *Communicated by* W. H. Wollaston, *M. D. Sec. R. S.*

Read May 11, 1815.

THE Royal Society did me the honour of printing in the volume of their Transactions for last year, a paper of mine on the nature of the salts termed triple prussiates, and on acids formed by the union of certain bodies with the elements of the prussic acid.

In that paper, I endeavoured to prove, that the elements of the prussic acid would combine with a certain proportion of black oxide of iron, and form a peculiar, and hitherto unknown acid, for which I proposed the name of the ferruretted chyazic acid. I showed that this was the real acid portion of the salts which had received the erroneous appellation of triple prussiates, and that the property of combining with the prussic acid, so as to change its nature, and increase its acid properties, was not confined to the black oxide of iron, but was possessed probably by many other bodies, but certainly by sulphur, which formed with it another acid, for which I proposed the name of the sulphuretted chyazic acid. The paper also contained some analytical researches into the proportions

in which the elements of these new acids are combined in them, and also into the proportions in which they unite with different saline bases.

My object in this paper, is to add to the analyses contained in the former, two analyses which I have since made; and then to apply to the whole, the admirable theory of Dalton, by which the proportions in which bodies can combine, are conceived to be governed by the relative weights of their chemical atoms, and also BERZELIUS's addition to this theory, by which the combinations of oxides with one another, are conceived to take place in such a manner, that the oxygen contained in one of these bodies, is either equal to, or is a multiple by a whole number, of the oxygen contained in the others.

I begin with describing the two analyses to which I have just alluded.

Analysis of prussiate of mercury.

A. Fifty grains of this salt finely pulverised, were kept at the temperature of 212° for six hours, at the end of which time, they weighed exactly the same as before.

B. Forty grains of this salt were dissolved in water and decomposed by hydro-sulphuret of potash: the products of this decomposition were prussiate of potash and black sulphuret of mercury; the quantity of the former could not be ascertained with accuracy, owing to the escape of much of the prussic acid, but that of the sulphuret amounted to 37.2 grains.

C. Disappointed in my attempt to estimate the quantity of prussic acid by the last experiment, owing to its very volatile nature, I availed myself of the property I had discovered

in the hydroguretted sulphurets, of converting the prussic acid at the moment they detach it from prussiate of mercury, into sulphuretted chyazic acid; which being much less volatile, and having a stronger attraction for alkaline bases than the prussic, could not escape from the liquid, and would give me the quantity of prussic acid it represented, by deducting from its weight, that of the sulphur which I knew to exist in it. I therefore dissolved 10 grains of prussiate of mercury in hot water, and poured hydroguretted sulphuret of soda into the solution until it no longer occasioned a black precipitate. This black precipitate when dry, weighed 9.3 grains; to the liquid from which it was separated, I added a few drops of diluted sulphuric acid; these caused a separation of a minute quantity of sulphur, which was got rid of by subsidence, after which I poured into it an aqueous solution of the two sulphates of copper, and of black oxide of iron, in which the former salt was to the latter by weight, as 2 is to 3, until no farther effect was produced. By these means I threw down the whole of the sulphuretted chyazic acid contained in the liquid, and collected it combined with protoxide of copper, in the form of an insoluble white salt, which weighed 9.7 grains.

But as 100 grains of this salt contain 40.62 grains of sulphuretted chyazic acid, composed of 26.39 sulphur and 14.23 prussic acid, according to my analysis, Phil. Tran. for 1814, page 549, Exp. C, (corrected by calculations in the Table facing page 230 of the present paper), therefore the before mentioned 9.7 grains represent 1.38 of prussic acid, which according to this experiment is the quantity existing in 10 grains of prussiate of mercury.

D. I had next to ascertain how much red oxide of mercury was represented by the 37.2 grains of black sulphuret obtained in Experiment B, and by the 9.3 grains of the same substance obtained in Experiment C. In order to effect this, I made the following experiment: 25 grains of corrosive sublimate were dissolved in water, and decomposed by hydro-sulphuret of potash; the black sulphuret thus formed, weighed 21.5 grains, which, therefore, represents 19.94 grains of red oxide of mercury, that being the quantity contained in 25 grains of corrosive sublimate.

Then as 21.5 : 19.94 :: 37.2 : 34.48 the quantity of red oxide in 40 prussiate of mercury,

And as 21.5 : 19.94 :: 9.3 : 8.62 the quantity of ditto in 10 of ditto,

100 grains of prussiate of mercury are therefore composed of

Prussic acid, Experiment C. - -	13.8
Red oxide of mercury, Experiment B. C. and D.	86.2
	100.0

Analysis of prussic acid.

Being very desirous of accomplishing the analysis of this acid if possible, I considered very attentively the nature of the difficulties to be surmounted in order to effect it. The principal ones appeared to me to be the following.

1st. That of always ascertaining with precision, the quantity which is the subject of analysis.

2d. That of effecting its combustion with oxygen in such a manner, that while on the one hand, the whole of its carbon and hydrogen should be oxygenated, so on the other, that none of its azote should undergo this process.

3d. That of determining with great accuracy, the quantity of oxygen which combines with the elements of the prussic acid during its combustion, so as after allowing for what has been expended in the formation of carbonic acid, to be able to infer with confidence, from the disappearance of the rest, the quantity of hydrogen which was contained in the acid.

The property which the prussic acid possesses of assuming the liquid form at a low temperature, and that of a gas or vapour at common temperatures, the volume of which is materially influenced by mixture with other gases, and by slight alterations of temperature and pressure; did not appear to me to be favourable to the employment of it in an uncombined form for the purpose of its analysis.

I therefore determined upon employing it in the state of condensation in which it exists in prussiate of mercury, and this determination made me undertake the analysis I have just described of that salt; of the correctness of which, having satisfied myself, I conceived that I had overcome the first difficulty.

The second and third difficulties I thought would be best surmounted by employing, for the combustion of the prussic acid, the same oxide with which it is united in the prussiate of mercury, namely, the red oxide of that metal; increasing the quantity of it by multiples of that which the salt contains, until I found that the whole of the prussic acid was decomposed.

I made a number of experiments upon this plan, the results of which proved to me that the quantities of carbonic acid and of azote gases produced, did not arrive at the maximum until five times the quantity of red oxide of mercury contained

in the prussiate had been added to it, making together six of that oxide, to one of prussic acid, and that whenever a less quantity of the oxide than this had been employed, there always existed in the gaseous products, a portion of undecomposed prussic acid. I farther observed, that in all cases the volume of azote gas obtained, was exactly equal to that of the prussic acid decomposed, that the volume of carbonic acid gas produced was invariably twice that of the azote gas liberated in the same experiment, and that the carbonic acid produced accounted for only one third of the oxygen consumed. The observance of these laws by which the decomposition was regulated, enabled me in constructing the following Table (facing page 228,) to correct the minute and unavoidable inaccuracies of experiment, by the superior accuracy to be acquired by applying to the results so obtained, the corrections necessary to make them correspond with the above-mentioned laws. It enabled me also to represent in the column denoting the measures of prussic acid gas, equal quantities by equal bulks; which, for the reasons before stated, experiment does not exactly show, and thus to render evident the true progress of its decomposition.

It may be proper before proceeding farther, to describe my mode of operating, in conducting the experiments from which the Table was compiled. This mode is similar in principle to that invented by GAY LUSSAC and THENARD in their Analysis of Animal and Vegetable substances, and improved by BERZELIUS. I am greatly indebted to these two French chemists, for the valuable information respecting this kind of analysis, which I have obtained from their Recherches Physico-Chymiques, and to Dr. BERZELIUS for that which I have received

from his Paper on the definite proportions in which the elements of organic nature are combined, published in Dr. THOMSON's Annals of Philosophy for December last. It is to this information that I principally attribute the success which has attended the experiments of a similar nature, which I have made.

The method pursued by me, however, differs in several respects from that of either of the chemists just mentioned,

1st. In the apparatus employed, which is much more simple in my process

2dly. In the nature of the oxygenised body employed to effect the combustion.

3dly. In the method to which I had recourse, for proportioning the oxygenised to the combustible body, by making the former a multiple of that which enters into chemical union with the latter.

4thly. In decomposing a much less quantity of the combustible body at a time, than either of the above chemists.

In the present case, each of these alterations appeared to me to possess very decided advantages over the other methods. How far they may be applicable to other cases, I do not pretend to determine.

Having thus generally stated in what my process differed from former ones, I proceed to rather a more particular description of it.

I prepare the peroxide of mercury which I employ, by decomposing with pure soda, a solution of corrosive sublimate. Having weighed out the proportions of prussiate of mercury, and of the peroxide which I intend to decompose, I triturate them together in a small polished mortar of porphyry or agate

for several minutes, then collect into the centre of the mortar, what adheres to its sides, and repeat this alternate trituration and collection at least six times.

I then take a tube of glass 4½ inches long, about the size of a common writing quill and tolerably stout, I close one end of it, and bend the other round, so that nearly an inch of that end forms a right angle with the rest. I call this the retort tube. I make a second tube similar to the first, except that instead of being closed at one end, it is open at both. I call this the adapting tube.

The retort tube is then charged with the mixed materials, by means of a small paper funnel, fixed with sealing wax to the top of the tube; the charge is introduced in about three equal portions, each of which is separated from the others by the introduction of a little coarsely powdered green glass; the charge generally occupies about 2½ inches of the tube. After its introduction, the wax which fastened the funnel is softened by heat, and the funnel detached.

A graduated glass tube, capable of containing about 2½ cubic inches, was next filled with mercury, and placed in the mercurial pneumatic trough, not in the usual perpendicular position, but with its upper end raised, but very little, from the horizontal situation, being about an inch above the surface of the mercury, while its lower or open end just dipped below that surface. In this position, the long leg of the adapting tube was passed up into it, which being open at both ends became filled with mercury; the short end of this tube was then connected with the short end of the retort tube, by means of a caoutchouc tube firmly tied to both. The long end of the retort tube when thus disposed, hung over the outside of

the end of the mercurial trough, in a position declining a little from the horizontal one towards the table. The decomposition was then commenced, by applying the flame of a spirit lamp to the empty part of the tube, and bringing it down gradually, so as to explode in succession the three strata of the mixture. The arrangement of the apparatus at the commencement of the process, will be instantly seen by an inspection of the annexed sketch.

When the retort tube was cold, it was separated, under the surface of the mercury, from the adapting tube, in such a manner, that any gas remaining in the latter might pass up into the graduated tube; the volume of gas collected, was then ascertained, making the necessary corrections for temperature, pressure, and the capacity of the retort tube, after which a solution of pure potash was passed up into it, and the diminution of volume which it occasioned was noticed; from the gas which remained, a deduction was made, for the quantity of atmospheric air in the upper part of the retort tube before the combustion, and which seldom exceeded $\frac{1}{30}$ of a cubic inch; the residual gas was considered as azote, and found to be so by all the tests to which I subjected it. The small quantity of solution of potash employed to effect the absorption was then examined, and if, besides carbonic acid, it was found to contain prussic acid, I concluded that I had not employed enough of the red oxide of mercury in the

[*To face page* 228.

TABLE showing the results of the decomposition by heat of prussiate of mercury, by itself, and also when mixed with multiples by whole numbers of its base.

Materials before decomposition.							Products after decomposition.													
Prussiate of mercury and red oxide of mercury.							Weight of oxygene in oxide of mercury.	Total weight of materials.	Gases.								Water.	Mercury.	Weight of oxygen in products.			Total weight of products.
									Prussic acid.		Carbonic acid.		Azote.		Total.				In the carbonic acid.	In the water.	Total.	
							Grs.	Grs.	C. In.	Grs.	C. I.	Grs.	C. I.	Grs.	C. I.	Grs.	Grs.	Grs.	Grs.	Grs.	Grs.	Grs.
Grs.		Prussic acid.		Red. ox. merc.								*		†								
2.5 of prussiate of mercury or Red oxy. merc.		0.344	+	2.155			0.159	2.5	0.395	0.2866	0.258	0.0732	0.079	0.0234	0.632	0.3832	0.1205	1.995	0.053	0.106	0.159	2.4987
Ditto, with 2.155	-	0.344	+	4.31			0.319	4.655	0.316	0.2293	0.316	0.1463	0.158	0.0467	0.790	0.4223	0.2410	3.991	0.106	0.213	0.319	4.6543
Ditto, with 4.31	-	0.344	+	6.465			0.479	6.81	0.237	0.1719	0.474	0.2195	0.237	0.0701	0.948	0.4615	0.3615	5.986	0.160	0.319	0.479	6.809
Ditto, with 6.465	-	0.344	+	8.62			0.638	8.965	0.158	0.1146	0.632	0.2926	0.316	0.0934	1.106	0.5006	0.4820	7.982	0.213	0.425	0.638	8.9646
Ditto, with 8.62	-	0.344	+	10.775			0.798	11.12	0.079	0.0573	0.790	0.3658	0.395	0.1168	1.264	0.5399	0.6025	9.977	0.266	0.532	0.798	11.1194
Ditto, with 10.775	-	0.344	+	12.93			0.957	13.275	0.000	0.0000	0.948	0.4389	0.474	0.1401	1.422	0.5790	0.7230	11.972	0.319	0.638	0.957	13.274

* Weight of carbonic acid taken as 0.463 grain for a cubic inch.

† Weight of azote taken as 0.2956 grain for ditto.

combustion, and repeated the experiment with an increased proportion of it.

Such was my method of effecting the analysis of the prussic acid, and by which as will be seen in the last line of the Table, I succeeded in discovering that 0.3442 gr. of it were composed as follows:

Carbon	= to that in 0.4389 gr. of carbonic acid, or	0.1198
Azote	= to the weight of the azote gas collected	0.1401
Hydrogen	= to that in 0.7230 gr. of water -	0.0843
		0.3442

consequently that 100 grains contain

Carbon - -	34.8
Azote - -	40.7
Hydrogen -	24.5
	100.0

Having finished my analytical investigations, I pass on to the last divison of my subject which is the following comparative view of the composition of the prussic, ferruretted chyazic, and sulphuretted chyazic acids, and of their salts, as deduced from my analytical experiments, and as inferred from the atomic theory.

I was very well aware of the probability of my placing some of my analyses in a very unfavourable light, by contrasting the results obtained by the application of a theory, capable of giving the composition of bodies with absolute certainty, with those results which I have obtained by practical experiments, on a class of bodies hitherto little examined,

or understood, and the analyses of which were very difficult: but I would not allow this consideration to have any influence in deterring me from making such a contrast, for as I had not the vanity to give these analyses as perfect, so I feel no mortification in now proving, that they were not so; and being confident that I had not spared either time or trouble in making them, I expose their imperfections without hesitation, confiding in the candid judgment of those, who, having undertaken similar investigations, are aware of the numerous difficulties, and sources of error attendant upon them.

I have arranged and collected these comparisons into the form of a Table, which I beg leave now to introduce.

I infer from the Table, that the acids and salts included in it, are so composed as to harmonize perfectly with the doctrines of DALTON and BERZELIUS, and to be very compatible with the opinion respecting the compound nature of azote.

I shall be happy if this attempt to elucidate the nature and composition of these bodies, adds in any degree to the daily and rapid progress now making in chemical science.

ROBERT PORRETT, Jun.

Tower, Feb. 22, 1815.

TABLE showing in what degree the results of my analyses coincide with the atomic theory

		Azote.	Carbon.	Hydrogen.	Prussic acid.	Red oxide of mercury.	Prussiate of mercury.	Sulphur.	Sulphuretted chyazic acid.	Protoxide of copper.	Sulphuretted chyazate of protoxide of copper.	Peroxide of copper.
Prussic acid. [1]	By analysis per cent.	40.7	34.8	24.5	100.	—	—	—	—	—	—	—
	By theory per cent.	40.7047	34.8169	24.4784	100.	—	—	—	—	—	—	—
	By theory per weight of atom.	1 atom	2 atoms	8 atoms	1 atom	—	—	—	—	—	—	—
		27.56	15.02	10.56	43.14							
Prussiate of mercury. [2]	Analysis per cent.	—	—	—	13.8	86.2	100.	—	—	—	—	—
	Theory per cent.	—	—	—	13.7766	86.2234	100.	—	—	—	—	—
	Theory per atom	—	—	—	1 atom 43.14	1 atom 270.	1 atom 313.14	—	—	—	—	—
Sulphuretted chyazic acid. [3]	Analysis per cent.	—	—	—	34.8	—	—	65.2	100.	—	—	—
	Theory per cent.	—	—	—	35.0333	—	—	64.9667	100.	—	—	—
	Theory per atom.	—	—	—	1 atom 43.14	—	—	4 atoms 80.	1 atom 123.14	—	—	—
Sulphuretted chyazate of protoxide of copper. [4]	Analysis per cent.	—	—	—	—	—	—	—	37.15	62.85	100.	—
	Theory per cent.	—	—	—	—	—	—	—	40.6215	59.3785	100.	—
	Theory per atom	—	—	—	—	—	—	—	1 atom 123.14	2 atoms 180.	1 atom. 303.14	—
Sulphuretted chyazate of peroxide of copper. [5]	Analysis per cent.	—	—	—	—	—	—	—	34.73	—	—	65.2
	Theory per cent.	—	—	—	—	—	—	—	38.11	—	—	61.8
	Theory per atom	—	—	—	—	—	—	—	1 atom 123.14	—	—	2 ato 200.
Sulphuretted chyazate of barytes. [6]	Analysis per cent.	—	—	—	—	—	—	—	30.7	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	38.7525	—	—	—
	Theory per atom	—	—	—	—	—	—	—	1 atom 123.14	—	—	—
Ferruretted chyazic acid. [7]	Analysis per cent.	—	—	—	63.79	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	66.569	—	—	—	—	—	—	—
	Theory per atom	—	—	—	4 atoms 172.56	—	—	—	—	—	—	—
Ferruretted chyazate of potash. [8]	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—
Ferruretted chyazate of black oxide of iron. [9]	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—
Ferruretted chyazate of peroxide of iron. [10]	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—
Ferruretted chyazate of barytes. [11]	Analysis per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per cent.	—	—	—	—	—	—	—	—	—	—	—
	Theory per atom	—	—	—	—	—	—	—	—	—	—	—

1 Page 229 of this Paper.
2 Page 223 of this Paper.
3 Phil. Trans. for 1814, page 549, C.
4 Ditto, page 555, B.
5 Inferred from the Analysis of the former
6 Phil. Trans. 1814, page 555, B.

[To face page 220.

eory of DALTON, and with the law regulating the combinations of oxidized bodies discovered by BERZELIUS.

Peroxide of copper.	Sulphuretted chyzate of peroxide of copper.	Barytes.	Sulphuretted chyzate of barytes.	Black oxide of iron.	Ferruretted chyzic acid.	Potash.	Water.	Ferruretted chyzate of potash.	Peroxide of iron.	Ferruretted chyzate of black oxide of iron.	Ferruretted chyzate of peroxide of iron.	Ferruretted chyzate of barytes.	Atoms of oxygen. In the prussic acid. Upon the supposition that azote is a simple body.	Upon the supposition that azote is an oxide of an unknown radical	In the oxide which is an essential part of an acid.	In the oxides which act as bases in the salts.	In the water contained in the salts.
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	2	—
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—	none	1	—	2	—
65.27	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
61.89	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
2 atoms	1 atom.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
200.	323.14	—	—	—	—	—	—	—	—	—	—	—	none	1	—	4	—
—	—	69.3	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	61.2475	100.	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	2 atoms	1 atom	—	—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	194.62	317.76	—	—	—	—	—	—	—	—	—	none	1	—	2	—
—	—	—	—	36.21	100.	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	33.431	100.	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	1 atom.	1 atom	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	86.66	259.22	—	—	—	—	—	—	—	none	4	2	—	—
—	—	—	—	—	47.66	39.34	13.	100.	—	—	—	—	—	—	—	—	—
—	—	—	—	—	45.706	42.318	11.976	100.	—	—	—	—	—	—	—	—	—
—	—	—	—	—	1 atom	4 atoms	6 atoms	1 atom.	—	—	—	—	—	—	—	—	—
—	—	—	—	—	259.22	240.	67.92	567.14	—	—	—	—	none	4	2	4	6
—	—	—	—	32.559	55.385	—	12.056	—	—	100.	—	—	—	—	—	—	—
—	—	—	—	34.632	51.796	—	13.572	—	—	100.	—	—	—	—	—	—	—
—	—	—	—	2 atoms	1 atom	—	6 atoms	—	—	—	—	—	—	—	—	—	—
—	—	—	—	173.32	259.22	—	67.92	—	—	500.46	—	—	none	4	2	4	6
—	—	—	—	—	53.38	—	11.62	—	35.	—	100.	—	—	—	—	—	—
—	—	—	—	—	49.8059	—	13.05	—	37.1441	—	100.	—	—	—	—	—	—
—	—	—	—	—	1 atom	—	6 atoms	—	2 atoms	—	1 atom	—	—	—	—	—	—
—	—	—	—	—	259.22	—	67.92	—	193.32	—	520.46	—	none	4	2	6	6
—	—	49.1	—	—	34.31	—	16.59	—	—	—	—	100.	—	—	—	—	—
—	—	49.629	—	—	33.051	—	17.320	—	—	—	—	100.	—	—	—	—	—
—	—	4 atoms	—	—	1 atom	—	12 atoms	—	—	—	—	—	—	—	—	—	—
—	—	389.24	—	—	259.22	—	135.84	—	—	—	—	784.3	none	4	2	4	12

former salt.

7 Phil. Trans. 1814, page 534, B and C.

8 Ditto, page 534.

9 Inferred from the analysis of the next salt.

10 Phil. Trans. 1814, page 537, A, B, and D.

11 Ditto, page 535.

XV. *On the nature and combinations of a newly discovered vegetable acid; with observations on the malic acid, and suggestions on the state in which acids may have previously existed in vegetables.* By M. Donovan, *Esq.* *Communicated by W. H. Wollaston, M. D. Sec. R. S.*

Read June 1, 1815.

Having often observed the sour disagreeable taste of the berries of the Sorbus (or Pyrus) Aucuparia, it occurred to me that the juice might contain an acid of a peculiar nature, and I resolved to submit it to a strict examination. I was not at that time acquainted with the fact, that these berries had already occupied the attention of Scheele, and that this philosopher had pronounced their acid to be the Malic.

Some preliminary experiments showed me that the juice occasioned a precipitation in acetate of lead and nitrate of silver; but it produced no change in lime water, barytes water, sulphate of copper, nor, although it might be expected from its very astringent taste, did it affect sulphate of iron.

A quantity of the juice was poured into a solution of sugar of lead; a curdy and somewhat heavy precipitate appeared, which was separated by filtration: this, from its solubility in acetic and dilute nitric acid, I judged to be malate of lead. The filtered liquor was red, and perfectly transparent, but after a few hours, I observed a powder deposited at the bottom, and as I saw no sufficient reason for its appearance, it

attracted my attention. To the naked eye it looked like a coarse powder; but, when examined by the microscope, proved to be composed of amorphous crystalline grains. With this small quantity of powder I made some trials, which ended in exciting farther curiosity. It was probable, that the precipitate which remained on the filter might be made to furnish more grains, and accordingly, after edulcorating the mass, I caused boiling water to pass through it. After two hours I examined the different washings; those made with cold water remained as at first, but those with boiling water had become white and turbid, from the suspension of a subtile powder. In several hours after, the cloud had disappeared, and displayed an abundant and beautiful formation of slender prismatic crystals, which glistened with silvery splendor at the bottom of the vessel. The mass which remained on the filter had become hard, was much diminished, and was very dense.

I accounted for the production of crystals by supposing that the precipitate, whatever might be its composition, was insoluble in cold water, but soluble in very large portions of boiling water, and hence the crystalline deposition on cooling. Experiment, however, showed, that the residual hard mass, even when levigated, boiled with water, and filtered, would no longer afford crystals on cooling: and this was a sufficient objection to my supposition; for, if one portion of the compound be soluble, why not the whole?

I observed, that even when the precipitate newly obtained, was washed with portions of cold water until it no longer altered vegetable colours; yet the first, second, and sometimes the third washing with boiling water would, after the deposition of the crystals, immediately redden litmus. An

opinion now suggested itself, which the following trials greatly strengthened.

In order to obtain the acid of the saturnine precipitate, the latter was boiled with dilute sulphuric acid: the liquor became red and transparent, while the sulphate of lead subsided to the bottom. Imagining that I had now obtained the acid in a state of sufficient purity, although coloured with adhering vegetable matter, I thought to separate the sediment by filtration, but was disappointed: for the liquor came through very turbid, notwithstanding that the supernatant liquor had been transparent. It was apparent, therefore, that the sulphuric acid being in too small a quantity, had displaced but a part of the vegetable acid, that the latter dissolved the remaining part of its own combination with lead, and deposited it when the solution touched any thing cold, thus producing the turbidness. After some hours, the turbidness in the filtered liquor subsided, forming a stratum on the bottom of the vessel, over which lay a number of crystals.

The theory of the crystalline formation in the washings of the precipitate now became obvious. It appeared that when the saturnine compound was washed with cold water, no other effect than edulcoration was produced: but that hot water partially decomposed the mass into a super and a sub-salt, the former of which being soluble in boiling water, filtered through, but on cooling, deposited crystals of the neutral salt, while free acid was left dissolved in the liquor. The first washing contained most free acid, and therefore suffered least of the neutral salt to crystallize.

The red acid liquor, as has been stated, contained much lead, and this it was necessary to separate. The most unex-

ceptionable method appeared to be the transmission of sulphuretted hydrogen through the clear liquor. This was accordingly done, after having heated the acid liquor so as to redissolve the crystals and sediment. The resulting sulphuret of lead was filtered off, and the clear fluid was boiled for a length of time, to expel the superfluous gas.

Supposing now that I had obtained the pure acid, I began to form various conjectures as to its nature; in the midst of which I discovered, that the berries of the Sorbus Aucuparia had already occupied the attention of SCHEELE, and that he had pronounced their acid to be the malic. There was indeed a great coincidence of properties between the two acids: malic acid is red, when evaporated to dryness it deliquesces, its combinations with potash, soda, and ammonia are deliquescent; such were also the properties of the acid under consideration. Yet I had never understood that malate of lead could be made to afford crystals: an experiment on this head, therefore, became necessary.

The juice of nearly ripe apples was saturated with potash, and the solution when filtered, was mixed with solution of acetate of lead: the precipitate was collected on a filter. This after being edulcorated was washed with boiling water, as before. In sixteen hours, crystals, precisely the same as the berries had afforded, were deposited, although less in quantity.

The production of crystals in both these cases, seemed to show that the acid of both fruits was the same: yet there was one difference. The precipitate remaining on the filter, after the action of boiling water, was, in that produced from apples, soft and pasty; but that obtained from the Sorbus berries was hard and dense. It became therefore necessary,

to discover what would be the habitudes of malate of lead obtained by other means.

Scheele showed that the primary action of nitrous acid on sugar is to form malic acid. I therefore heated together equal weights of these substances, until the effervescence ceased. The brown residue was diluted with lime water, and when the oxalic acid that might have been formed, was in this manner separated, the remaining acid substance was saturated with potash. Acetate of lead was added, and the malate of lead thus formed was collected and edulcorated.

It now remained to ascertain whether this malate possessed the property of crystallizing, like that prepared from apple juice. I accordingly poured on it different portions of boiling water which were received in different vessels: the washings were all of a brownish yellow colour, from a small quantity of malate of lead held dissolved. At the end of 48 hours this salt was all deposited in the state of a brown subtile powder, but there was no formation whatever of crystals. On the surface of each washing was an iridescent pellicle of some lustre, which I found to be characteristic of the malic acid. This experiment, as Scheele directs, was made with weak nitric acid: I repeated it with an acid of considerable strength, but after sixty hours there was not one crystal.

It deserves attention, that the matter which remained on the filter in these two experiments, after washing with boiling water, were as soft and pasty as when first collected; whereas the salt of lead obtained from the berries of the Sorbus, had grown dense, hard, and was much diminished.

The saturnine compound that had been formerly obtained

from the berries, when partially decomposed by sulphuric acid, afforded crystals on cooling: in order to ascertain if the malate of lead obtained from sugar would do the same, I repeated the process on it, but obtained no crystals. These experiments were made under almost every possible circumstance with the same results.

I next precipitated all the lead from the remaining malate; the malic acid thus obtained, was again mixed with acetate of lead, and the precipitate treated with boiling water as before: but the results were the same. From this malate of lead the acid was again separated, and the same process as before was four times repeated: but notwithstanding nearly all the foreign matter was thus separated, no crystals formed.

SCHEELE found that the gooseberry contains nearly equal parts of citric and malic acids. I thought it necessary to examine if this fruit would afford crystals. SCHEELE's process for separating the acids is as follows. The juice is to be evaporated to the consistence of honey, the pure acid to be separated by alcohol, and the alcohol to be finally evaporated. The residuum is to be dissolved in water, saturated with carbonate of lime; the solution is to be filtered, and boiled so as to separate the citrate of lime. The remaining brown solution is malate of lime: the salt is to be precipitated by alcohol, redissolved after edulcoration, and the solution is to be precipitated with acetate of lead.* All this I followed; but after treating the malate of lead with boiling water, I obtained no crystals.

The raspberry was found by SCHEELE to contain the same

* CRELL's Chemische Annal. 1785, vol. ii. 292.

acids. I examined this fruit in a similar manner, but no crystals were produced.

Vauquelin detected malic acid in a variety of plants, but in none so abundantly as the Sempervivum Tectorum. In the juice of this plant, it is united to lime in the state of a supersalt.*

Malate of lead was formed by pouring solution of acetate of lead into the filtered juice of this plant. The precipitate, when washed with boiling water, deposited on cooling, the the same subtile powder as usual, but no crystals. Nor were any obtained when the malate was partially decomposed by sulphuric acid, in the manner already described.

Scheele found that the berries of the Sambucus Nigra, contain the malic acid unmixed with any other. I therefore examined their very mucilaginous juice, but could not produce crystals.

As in my experiments on apples I had obtained crystals, I wished to try what effect the vinous fermentation might have on their juice. The process was repeated with cyder, but I was surprised to find, that although a malate of lead was obtained, the property of forming crystals had been destroyed.

From various conjectures it appeared possible, that the berries of the Sorbus while very young, might perhaps contain a malic acid that would furnish few or no crystals. In the beginning of August (1812) the berries being still green, I made an infusion of them, which after filtration afforded a precipitate with acetate of lead. Boiling water produced no crystals, nor did the mass become hard as on former occasions.

Seven varieties of malic acid had now been examined,

* Annales de Chimie, Tom. xxxiv. 127.

which when united to lead would not afford crystals. In the two latter cases it was surprising that those acids which in other trials afforded abundance, would now afford none. That the juice of the green berries was real malic acid, was proved by the following properties.

1. The acid, when separated from the juice, was of a crimson colour.

2. When evaporated to dryness, it soon deliquesced.

3. When rendered solid, and acted on by nitrous acid, some crystals of oxalic acid appeared.

4. The acid formed deliquescent salts with potash, soda, and ammonia.

5. Its combinations with lime and lead possessed their proper characteristics.

The acid of very young sour apples was combined with lead, and the compound treated with boiling water: the washings deposited much more crystals than mature apples would have done.

The truth was now manifest. Beside the malic, there is another acid formation, which in different fruits is formed at different periods, and which has hitherto escaped observation: and I consider the preceding details by no means redundant, inasmuch as they establish one decided difference between the two acids. Many others will shortly be stated.

The first step towards confirming the difference, was to obtain the new acid in a state of purity: and after numerous attempts, I found that the only way to obtain it pure, was to separate it from the crystals. The process is indeed complex and difficult.

The berries of the Sorbus Aucuparia are to be collected

when first they have arrived at maturity. After sufficient bruising in a Wedgewood mortar, they are to be subjected to strong pressure in a linen bag. If collected at the most favourable time, they will afford nearly one half their weight of juice, s. g. 1077. This after due subsidence is to be strained, and mixed with filtered solution of acetate of lead. The precipitate is to be collected on a filter, and in order to separate any uncombined colouring matter, it should be washed with cold water. A very large quantity of boiling water is to be poured on the filter, and allowed to pass through the precipitate into different glass jars. After some hours, the washings become opaque, and at length deposit crystals of singular lustre and beauty. Those which have been formed in the colourless washings are to be alone retained; they are to be separated by the filter, dried in the air, and preserved for a farther process.

The original mass remaining on the filter, from which the crystals have been obtained, being now hard and brittle, is incapable of affording any more, without undergoing a new operation. It is to be boiled for half an hour with a little more dilute sulphuric acid than is sufficient to decompose the salt; when cold it is to be filtered. The filtered liquor is to be mixed a second time with acetate of lead: the precipitate washed, as before, with boiling water, and the crystals selected from the colourless washings only. The remaining mass again grown hard, is to undergo the process of decomposition with sulphuric acid, combination with lead, and the formation of crystals: and after all, it will be found that the crystals of all the processes will be inconsiderable when collected.

The whole of the crystalline product being dried, is to be

boiled for half an hour with 2.3 times its weight of sulphuric acid, s. g. 1090, supplying water as fast as it evaporates, and taking care to keep the mixture constantly stirred with a glass rod. The clear liquor is to be filtered off, and poured into a tall glass jar of small diameter. While still hot, a stream of sulphuretted hydrogen is to be transmitted through it, and when all the lead has been precipitated, the fluid is to be filtered off, and boiled in an open basin, until the discharged vapour no longer blacken paper impregnated with acetate of lead.

The theory of the process is obvious. When acetate of lead is added to the juice, malate of lead and the combination of the new acid with lead precipitate; the latter is decomposed by boiling water into a super and a sub-salt; the super-salt is held in solution, but as the liquor cools, the neutral compound deposits itself in crystals, and the first washings contain most free acid. When boiling water is no longer able to overcome the attraction of the latter portions of acid to oxide of lead, no more crystals can be formed. We then apply the stronger power of sulphuric acid, we obtain the free vegetable acid, and proceed as at first. When all the crystals are collected, such a quantity of sulphuric acid is added as will be *nearly* sufficient to decompose them: this is so done in order completely to exclude the sulphuric acid, which without this precaution would be exceedingly difficult to effect. The undecomposed portion of the crystals dissolves in the vegetable acid newly extricated: but, if in the boiling, the fluid were not continually stirred, a mass would be formed in the bottom so hard as to resist decomposition. If the liquor after filtering were allowed to cool, the neutral salt

would crystallize; it must therefore be used hot. The stream of sulphuretted hydrogen passing through so high a column of fluid soon separates the lead, while the pure vegetable acid is liberated, contaminated indeed with a little sulphuretted hydrogen. This gas does not disappear completely by boiling, for the acid retains the odour, be it ever so long boiled; exposure for a few days in an open vessel dissipates it completely.

In preparing this acid, it is not necessary, as it is in the process for malic acid, to saturate the juice of the berries with potash, at the commencement of the process: for of the two compounds formed after the mixture with acetate of lead, the malate dissolves in the evolved acetic acid in preference to the other. The colouring matter, which adheres obstinately to the malate of lead, is very apt, when extricated during the washing with boiling water, to tinge the otherwise perfectly colourless crystals, which form as the liquor cools. This is a great inconvenience, for the colouring matter cannot be washed away, even by cold water, without decomposing a quantity of the salt: hence the only remedy is, to reject all the crystals formed in the coloured washings, and to reserve those only that are of a pure whiteness. The crystals are of so delicate a fabric that they must be separated by the filter. When dried on paper, by exposure to air, they form a white brilliant flake of a silver lustre, resembling well-prepared acetate of mercury, but still more beautiful.

As to the amorphous crystalline grains which first attracted my attention, the following experiment elucidates the theory of their formation. A quantity of the pure acid obtained by the above process, was boiled for some time on an excess of

carbonate of magnesia. The liquor after filtration was found to restore the original colour of reddened litmus, and to render green the tincture of cabbage. An acetate of lead was formed by boiling solution of super-acetate on carbonate of lead. This solution was mixed with the former, and the precipitate was collected by the filter.

Notwithstanding the evident excess of magnesia existing in one solution, and the necessary neutrality of the other, yet the filtered liquor was found strongly to redden litmus paper. We are not to suppose that the two salts evolved a free acid during their mutual decomposition. As much oxide of lead was liberated from the acetate, as was necessary to the neutrality of the acid eliminated from the compound with magnesia; the solution would therefore have retained all its ingredients in a state of neutrality, except that which originally contained an excess of magnesia. But the new salt of lead at the moment of its formation, was decomposed by the water present into a super and a sub-salt: the excess of acid being not only sufficient to saturate the redundant magnesia, but also to leave a portion free in the solution. This liquor, after an hour, deposited a quantity of crystalline grains, and after that, the acidity increased. Hence appears the reason of a crystallization in the original liquor: a super-salt is formed, which after a while deposits the neutral salt in a crystalline form.

This acid appearing from what has been already stated, as well as from what will be hereafter detailed, to be of a peculiar nature, it became necessary to give it a name. After some consideration I bestowed on it one, which, although not unexceptionable, is sufficiently accordant with the general

analogy of chemical nomenclature, and which has received the approbation of some competent judges. Until a better name be devised, I have called it the Sorbic Acid.

To establish its peculiar nature, I have examined its combinations with certain bases, but have confined myself to those of which the analogous combinations amongst the malates had been already examined by SCHEELE. The sorbic and malic acids not having been distinguished by that philosopher, it seemed that here the distinction ought particularly to be established: and the standard of comparison must necessarily be whatever had been ascertained of the malates by their discoverer.

Sorbic acid, when perfectly pure, is a transparent, colourless, and inodorous fluid, soluble in alcohol, and in any proportion of water. When evaporated, it forms an uncrystallizable solid mass which deliquesces: when subjected to distillation, the liquor which passes over, shows no traces of acidity. Its acidity is such that it causes even a painful sensation on the organs of taste. It is not much altered by being kept in an uncombined state. I have had it for more than a year in a corked phial, and at the end of that time, no other change was produced than the separation of a tenuous coagulum, small in quantity, as the acid was very pure, but it is more abundant when the acid is impure. When mixed with malic acid, as in fruits, this acid is the first to disappear, while the other retains its properties long after the commencement of decay in the plant.

A quantity of malate of lead obtained from Sempervivum Tectorum was boiled with sorbic acid and a little water; the whole, from being colourless, became somewhat brown. The

liquor was then filtered, and the turbid liquor which came through, was heated until it became clear; it was then suffered to rest. As it cooled, it let fall a powder, but when this was filtered off, the liquor remained clear, and in an hour a great profusion of crystals was let fall. The mass which remained on the filter contained some gritty particles.

Thus it is evident, that malate of lead was decomposed by sorbic acid, which could not happen unless the latter were a distinct substance. The malate was only partly decomposed, the oxide of lead united to the excess of sorbic acid forming super-sorbate of lead, while the disengaged malic acid dissolved as much as it could of the remaining undecomposed malate, forming super-malate of lead. The brown colour was produced by disengaged malic acid. The super-malate, as it cooled, deposited its malate in the state of powder, and the super-sorbate soon after deposited sorbate of lead in the state of crystals; and the original mass was found to contain gritty particles of sub-sorbate.

In the same manner, when a precipitate, obtained by acetate of lead from the juice of the Sorbus berries, is washed with boiling water, scarcely any malate of lead is deposited; and if the fluid contain much free sorbic acid, the iridescent pellicle, which is a characteristic of malate of lead, does not appear.

I shall now proceed to the combinations of this acid, so as to distinguish it from malic acid: and first the salts which it forms with lead should be briefly recapitulated.

The sub-sorbate is insoluble in water; if in a mass, it is dense and hard; if in powder, it is gritty.

The neutral sorbate, if obtained by precipitation, is a white powder, but if obtained from solution in its own acid, it is in

beautiful silvery crystals. Neither of these salts is soluble in 5000 times its weight of water. The sorbate when heated to redness, undergoes a somewhat brilliant combustion.

The super-sorbate never assumes the solid form; its taste is sweet. Thus the sorbic acid forms three combinations with lead: malic forms but two, the neutral malate, which is an uncrystallizable soft powder, and the super-malate. Not less distinguishable are the two acids by their combinations with the alkalies.

Sorbate of potash, when there is an excess of acid, forms permanent crystals soluble in water, but insoluble in alcohol.

Sorbate of soda, when there is an excess of acid, forms permanent crystals, which agree in characters with the former.

Sorbate of ammonia, when there is an excess of acid, also forms permanent crystals of similar characters with the preceding.

These three salts will not crystallize unless there be a tolerable excess of acid; they are to be considered as super-salts. That of soda even requires the aid of cold to render it solid. The malates of potash, soda, and ammonia are known to be uncrystallizable and deliquescent.

In the combinations of these acids with earths, there are also striking differences. Scheele found, that when he added carbonate of lime to malic acid, a great quantity was dissolved, but the solution gave with litmus, indications of an abundant acid which it was impossible to neutralise with farther additions of chalk.* I obtained the same result with malic acid from the Sempervivum; I even found that the solution might be boiled to dryness, on a fresh portion of carbonate of lime,

* Scheele, Chem. Annal. 1785. 2. 292.

yet when lixiviated, the filtered solution would still redden litmus, and the salt finally afforded was readily soluble. These results often obtained, prove that it is not possible to form neutral malate from carbonate of lime.

But with sorbic acid the case was quite different. When it was diluted, and agitated for a little while with carbonate of lime, the solution, before it could be filtered, deposited the principal part of the sorbate in the form of a discrete, gritty powder. The liquor when filtered produced no redness in tincture of litmus, and every thing proved that the fluid by mere agitation over the carbonate, had been completely neutralised.

The same results which SCHEELE obtained from lime were afforded by carbonate of barytes: but with sorbic acid I produced a liquor which showed no signs of acidity. The best test for ascertaining this fact, seemed to be infusion of brazil wood altered by distilled vinegar; and with this it even appeared, that the solution contained an excess of base.

Thus it appears, that malic acid never forms with carbonate of lime, any other than *acidulous salts*; and, as SCHEELE observes, these solutions in *some days* deposit the neutral salt in *crystals*. But with these carbonates, the sorbic acid forms *neutral salts*, which, *as soon as formed*, precipitate.

SCHEELE ascertained, that malate of magnesia is a deliquescent salt,* and in my trials I could not obtain it in a crystalline form. When evaporated, it became thick, and dried into a semitransparent substance, which softened with the smallest quantity of water, and formed matter of a syrupy consistence. The same earth, heated in sorbic acid, afforded a

* CRELL's Chem. Annal. 1785, 2. 297.

liquor which, after filtration, deposited permanent crystals in abundance: they required for solution no less than 28 parts of water at 60.*

The malate of alumina was found by SCHEELE to be a salt very difficult of solution. I wished to discover the properties of the sorbate. I therefore boiled some very pure alumina that had been just prepared, and was therefore still soft, with sorbic acid: the boiling was continued for almost an hour, and after filtration, I discovered with no small surprise, that the alumina had not been acted upon. The acid was tried by every means, and nothing but the most minute vestiges of the earth could be obtained. Thus there is no sorbate of alumina.

I consider that from this property the sorbic acid may become a valuable instrument of analysis. The process for separating alumina from other earths, has been complicated and uncertain: may it not be rendered simple by the use of this acid, employed in excess?

Thus, I think there can be no doubt, that the sorbic acid is an acid *sui generis*, and probably intermediate between malic and oxalic. With regard to the other acids, with which the sorbic coexists in fruits, it is to be observed, that it is never found in mature fruits that contain any other than the malic; that the latter is never found alone in any mature fruit, but always accompanied by the sorbic, and that these two acids, when together, exclude all others. To this, however, there is an apparent exception, namely, the berry of the Sambucus Nigra, which (probably from the immense quantity of mucilage and

* It deserves remark, that in SCHEELE's experiments, there could have been no sorbic acid present, as might have been expected, had he prepared his acid from apples: he obtained it from gooseberries, and thereby avoided this source of fallacy.

colouring matter present) afforded me no sorbic acid. The fruits that contain the sorbic and malic acids together are apples, plums, berries of the sorbus, barberries, and sloes. Of these, the berries of the sorbus contain the greatest quantity of sorbic acid, unripe apples less, ripe apples and sloes still less, barberries very little, and plums least of all. The green berries of the sorbus, (perhaps,) those of the sambucus, and the plant sempervivum tectorum, contain no other than the malic; and agreeing with the foregoing statements, raspberries and gooseberries, as they contain citric and malic acids, contain no sorbic whatever.

Observations on the malic acid.

In 1785, during an examination of different fruits and berries, SCHEELE discovered that gooseberries, beside lemon acid, contained one of a peculiar nature: this he afterwards found to exist in apples, without, as he thought, a sensible admixture of any other. On this account he gave it the name of apple acid, or malic acid.

He also ascertained, that by the action of nitrous acid on sugar, a substance is produced, which shows no traces of nitric acid, yet unites and forms a soluble salt with lime, "it therefore is not the oxalic acid." By some other experiments he found that an acid is produced "which does not differ in the least from the properties of the apple acid, and is accordingly the same."

This acid he detected in a great variety of vegetable juices. Since that period, VAUQUELIN has extended the catalogue, but of all other plants, it is most abundantly contained in the Sempervivum Tectorum.

SCHEELE's process for obtaining malic acid is as follows.

"Saturate the juice of apples, whether ripe or unripe, with carbonate of potash; add solution of acetate of lead until it cease to produce a precipitation. To the edulcorated precipitate, add as much dilute vitriolic acid, as is necessary to give the mixture a perfectly acid taste, without any sweetness."*

There are several objections to this process, all of which seem to have considerable weight. In the preceding pages I have shown, that the juice of apples, whether ripe or unripe, always contains two acids of very different properties. By the above process these acids are not separated; they are in fact found in what is supposed to be the resulting pure malic acid, and it is impossible, without the most complicated processes, to obtain this substance in the insulated form.

The precipitation of the lead by means of sulphuric acid, appears to be objectionable. I have often attempted to adjust the proportion of the latter substance, so as to throw down all the lead, without leaving any free sulphuric acid, but I uniformly failed: and it is evident, that, if not impossible, it is exceedingly difficult and troublesome.

SCHEELE also attempted to obtain malic acid from malate of lime, by means of sulphuric acid, but found "the mode rather difficult, as the acid would not let the calx fall completely." VAUQUELIN observed the same thing.

The last process proposed by SCHEELE is to distil equal parts of weak nitric acid and sugar, until the mixture become brown, which is a sign that all the nitrous acid has been abstracted: the oxalic acid formed, is to be separated by lime water, and the remaining acid to be saturated with carbonate of lime.

* CRELL's Chemische Annal. 1785. Vol. II. 295.

The solution is to be filtered, the filtered liquor to be mixed with alcohol, and the coagulum thus obtained, is to be edulcorated with new portions of alcohol. The coagulum is then to be dissolved in water, and mixed with a solution of acetate of lead: a precipitate falls, which is to be treated with sulphuric acid, in the manner already directed.

No one who has not gone through this process, can fully conceive the difficulty and expense of it: and I have found that the acid obtained is variable in its nature. In one case I obtained an acid, which, when mixed with solution of acetate of lead, did not at first produce any effect, but at length slowly deposited a precipitate. The heating of another portion of the acid with carbonate of lime, produced a separation of a black powder, which possessed the properties of charcoal. There were also many other peculiarities; and the combined effect of all was to convince me, that great differences exist between the acid obtained in this manner, and that obtained by other processes.

The experiments of VAUQUELIN satisfied him that the acid which is combined with lime in the Sempervivum Tectorum, is the true malic: and all my trials convince me, that it does not contain even the least quantity of the sorbic. Since then, by the means generally employed, we do not obtain malic acid, the only alternative is to adopt the hitherto neglected process of VAUQUELIN; and it will be found that his process affords the acid with greater ease, and in much greater purity, than any other. The method of detaching the acid from the malate of lead by sulphuric acid is, as we have seen, difficult; and the criterion of the taste is liable to this fallacy, that as the sourness increases, the sweetness decreases. There will

on this account, be a period when the latter will disguise the former, and yet the lead will be still present. I would therefore suggest the substitution of sulphuretted hydrogen in place of sulphuric acid.

If it were required to obtain malic acid exceedingly pure, and still more divested of vegetable matter, the following process may be adopted.

The juice of Sempervivum Tectorum is to be evaporated to two-thirds, and, after standing some hours, it is to be filtered, and mixed with an equal quantity of alcohol. The coagulum is to be separated by the filter, edulcorated with fresh portions of alcohol, and dried in the air, lest any adhering alcohol should impede its subsequent solution. The mass is then to be dissolved in water, mixed with solution of acetate of lead, and the precipitate collected on a filter. After being well edulcorated from any superfluous acetate of lead, the precipitate is to be boiled for 15 minutes with a little less of dilute sulphuric acid than is sufficient to saturate the oxide of lead: and for this part of the process, the criterion of sweetness will very well answer the purpose. The whole is to be set aside for some days, and, during this period, a small quantity of sulphate of lead which the malic acid held dissolved, will be deposited. The liquor is now to be filtered, and in order to separate the last portions of lead, a stream of sulphuretted hydrogen is to be transmitted through it: the black precipitate is to be filtered off, and the liquor should be boiled in a vessel freely exposed, until paper moistened with acetate of lead is no longer blackened by the discharged vapour. This acid is the purest that can be obtained; it retains

a slight odour of the gas, but even this is destroyed by exposure to the air for a few days.

VAUQUELIN observes, that malic acid thus obtained is nearly colourless: his was therefore diluted. I have found that it becomes perfectly brown by concentration: and I have decomposed and recomposed malate of lead several times, using each time the same specimen of malic acid, yet so obstinately did the colouring matter adhere, that it was always found in the resulting acid. Thus, as far as we know, this acid cannot be procured free from colour; and the nearest approximation is that obtained by VAUQUELIN's process.

Suggestions concerning the state in which acids may previously have existed in vegetables.

I have sometimes indulged in the supposition, that the vegetable acids are not primarily formed by the immediate union of their elements, but that they may have previously existed in a definite combination, called the bitter principle. It is possible that this principle may be a compound basis, which by uniting to oxygen, or by undergoing more complicated processes, might change its nature so far as to become an acid. The whole is a mere conjecture, and perhaps deserving of little consideration; the facts, however, which suggested it may be noticed.

The sweetness of any vegetable juice, has been generally attributed to a sweet principle called sugar. In the same manner it has been lately supposed, that bitterness depends on a bitter principle, which, although variously disguised, is always identical. Dr. THOMSON has shown, that when

water is digested over Quassia, and afterwards evaporated to dryness, a transparent substance is obtained, which differs in its properties from all other vegetable principles: this he considers as the bitter principle, and, I believe, with very great justice. I found that the liquor obtained by digestion, although slightly coloured, was transparent even to the end of the evaporation. The resulting mass was nearly transparent, and minute in quantity, considering the proportion of Quassia employed; and such was its bitterness, that a particle placed on the tongue, which could not have exceeded $\frac{1}{50}$th of a grain, diffused an intense bitterness over the whole mouth and fauces.

This matter was heated with nitric acid; it dissolved with effervescence, and the bitterness was no longer sensible. The remaining substance formed a precipitate in acetate of lead, which possessed all the properties of malate of lead: and it appeared that no other than the malic acid was produced. With this experiment the following agrees in a remarkable manner.

Five ounces of white sugar, and an equal weight of very strong nitric acid were mixed in a retort. Without the application of external heat the action commenced, and soon became violent. When cold, the residual matter was found to be thick and tenacious; its taste was sour, and extremely bitter. The malic acid being abstracted from a portion of this by means of lime, it was found that the bitterness, now no longer disguised by acidity, had become intense. The other portion, which had not been saturated with lime, by being treated with more nitric acid, lost all its bitterness, and oxalic acid was formed. In this experiment it appears, that by some

action of nitrous acid on sugar, a bitter substance and malic acid were produced together; that by the farther action of nitrous acid, the bitter substance disappeared, and acid appeared in its stead.

The foregoing conjectures correspond also with the fact, that by the action of certain substances on each other, the bitter principle is evolved at the same time with those acids which I suppose to have been produced from that compound basis: and the appearance of both at the same time may be accounted for by admitting that the conversion was not complete. Thus, if alcohol be distilled with nitrous acid, a liquor is produced which has a sweet taste. If this liquor be re-distilled with another portion of acid, a bitter liquor comes over. And if this bitter liquor be distilled a third time with a fresh portion of nitrous acid, crystals of oxalic acid make their appearance in the residuum. This series of changes bears a striking resemblance to that produced by the action of nitrous acid on sugar.

HAUSSMAN observed, that when nitric acid is digested with indigo, a very bitter substance results, to which WELTHER gave the name of Amer: in this process, oxalic acid is also formed.

The vegetable acids are even formed by the action of nitrous acid on animal substances; in the instance of muscle we obtain the abovementioned Amer with oxalic acid. In bile the bitter principle is already formed; when acted on by nitrous acid, oxalic acid is produced.

On examination we shall not be at a loss to find operations analogous to some of the preceding, taking place naturally in the vegetable kingdom. The Pyrus Malus or common

crab apple, while young, is very bitter, and has little sourness: as the fruit advances towards maturity, the taste becomes proportionately sour, and the bitterness diminishes. The young berries of the Sorbus Aucuparia also are bitter, contain but one acid, and even that in small quantity: when the berry is ripe, it contains two acids, the combined quantity of both is considerable, but the bitterness has in a great degree given place to a coarse astringency.

It is not improbable, that the bitterness produced in all the foregoing cases, should be owing to the formation of the same bitter principle: and its constant conjunction with a vegetable acid, seems to show that there is some very intimate connexion between them, at present unknown.

The preceding observations are offered as mere conjecture; and I am fully sensible of what little consideration should be attached to them: they are not however entirely devoid of probability. An hypothesis is below the dignity of a system which is founded on the indestructible basis of experiment: and even though it be supported by the coincidence of admitted facts, by direct analogies, and by the correspondence of received opinions, it should nevertheless be the beginning and not the end of knowledge.

XVI. *On the structure of the organs of respiration in animals which appear to hold an intermediate place between those of the class pisces and the class vermes, and in two genera of the last mentioned class.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read June 1, 1815.

FROM all the facts in comparative anatomy with which we are acquainted, there is reason to believe, that the great scheme of the animal creation is composed of one uniform gradation of structures, and it is only by collecting the different appearances met with in the same organs of different animals, into regular series, that a solid basis can be laid on which a general system may be constructed.

In this view of the subject, every link that is added to any one series, acquires a value; as it increases, in however small a degree, the foundation upon which an edifice of such importance is to be raised, and therefore may not be undeserving of the attention of the Society.

In fishes, the mode of respiration by means of gills is well understood, and there is probably no better criterion by which an animal may be allowed to belong to that class, than its having gills. In the class vermes, confining our observations to those genera that live under water, the respiratory organs commonly met with, are of two kinds, one internal as in the genus Teredo, the other external as in the genus Amphitrite, both of which I have described to the Society upon a former occasion.*

* See Phil. Trans. Vol. lxxv. p. 333.

The materials of the present Paper include five several links in the chain distinct from the gills of fishes, and different from the organs common to vermes; these are met with in the lamprey and lampern, in a new genus intermediate between the lamprey and myxine, in the myxine, in the aphrodita aculeata, and in the leech.

I shall first give a short description of each of these organs, and afterwards explain the modes of respiration.

In the lamprey the organs of respiration have seven external openings on each side of the animal; these lead into the same number of separate oval bags placed horizontally, the inner membrane of which is constructed like that of the gills in fishes. There is an equal number of internal openings leading into a tube, the lower end of which is closed, and the upper terminates by a fringed edge in the œsophagus. These bags are contained in separate cavities, and inclosed in a thorax resembling that of land animals, only composed of cartilages instead of ribs, and the pericardium, which is also cartilaginous, is fitted to its lower extremity like a diaphragm.

In the middle line of the anterior part of the thorax are situated the muscles of the tongue, forming one solid mass, from which a distinct muscle is continued down to the pericardium, sending off fasciculi to the cartilages in the lower part of the thorax.

There is but one nostril, which opens into a cavity of considerable size, having no posterior opening. Where the œsophagus terminates in the stomach, it adheres to the pericardium, and forms an oblique valvular slit, which is closed by the dilatation of the stomach. There is no gall bladder.

In the lampern, the structure of these organs is the same

as in the lamprey, only the cartilages of the thorax are so weak as to appear like ligaments, and the pericardium is membranous.

In an animal brought from the South Seas by Sir JOSEPH BANKS, intermediate between the lamprey and myxine, but differing so much from both as to form a distinct genus, the respiratory organs resemble those of the lamprey in the number of the external openings, and the number of bags, but these organs and many other parts differ in the following particulars, in which they agree with those of the myxine. There is no appearance whatever of thorax, nor is the pericardium cartilaginous; the bags are flattened spheres placed perpendicularly, their cavities are small, their coats elastic, and the internal orifices communicate directly with the œsophagus, which is small. The œsophagus does not terminate in a valvular slit, but in a loose transverse membranous fold; there are two rows of teeth on each side of the tongue, bent downwards, long, and pointed. There is a posterior nostril, and an appearance resembling an uvula. There is a gall bladder, a row of large mucous glands on each side of the belly, and there is a mesentery to the intestine.

In the myxine, the respiratory organs differ from those last described, in there being only two external openings, and six lateral bags on each side, to which there are six tubes from each of the openings, and, close to the left external opening, there is one which passes directly into the œsophagus, the gall duct projects into the intestine.

In the aphrodita aculeata of LINNÆUS, the respiratory organs as well as the other viscera, differ in many respects from those of all the other animals of that tribe. There are thirty-two open-

ings on each side in the intermediate spaces between the tufts of bristles; these all open into a large cavity immediately under the skin and muscles of the back, which is only separated from the cavity of the abdomen by a strong cartilaginous membrane, but there are two rows of spherical cells, fifteen in each, projecting into the cavity with very thin membranous coats. There is no external opening into them, but a slit on the under surface, by means of which, one of the cæca belonging to each of the tubes passing off from the intestine is lodged in each of the cells, which leads me to consider such cæca to be the respiratory organs.

In the common leech, there are sixteen orifices on each side of the belly, which lead to an equal number of spherical cells placed between the abdominal muscles and the stomach, which perform the office of respiratory organs. The particular structures which have been described, are represented in the annexed drawings, (Plates XI. XII. XIII.) which makes a more detailed verbal description unnecessary.

Having described the structure of the organs in these five different genera of animals, I shall endeavour to explain the manner in which respiration is carried on in each.

In the lamprey and lampern, the water is received by the seven lateral openings on each side of the animal into the bags which perform the office of gills, and passes out by the same orifices. The form of the cavities being fitted to allow the water to go in at one side, pass round the projecting parts, and out at the other. A part of the water escapes into the middle tube, and from thence, either passes into the other bags, or out at the upper end into the œsophagus. There is a common opinion that the water is thrown out of the nostril;

this, however, is unfounded, as the nostril has no communication with the mouth. The elasticity of the cartilages of the thorax, admits the water being received, and it is expelled by the action of the muscles drawing up the cartilages and the pericardium. The animal from the South Seas having no cartilaginous thorax, the bags themselves have an elastic covering which keeps them open to receive the water, and it is expelled by the action of the external muscles into the œsophagus.

In the myxine, the elasticity of the two tubes and the bags into which they open, admits of the water being received, and the pressure produced by the action of the external muscles forces it into the œsophagus, from whence it is thrown out by the opening at the lower end of that tube.

BLOCH has given a correct account of many parts of the myxine, illustrated by engravings, but there are several errors respecting the mode in which the water passes out. He supposes it to be thrown out at the nostril. He was probably led into this mistake from finding a posterior nostril communicating with the mouth.

In the aphrodita aculeata, the water passes through the lateral openings between the feet into the cavity under the muscles of the back; it is there applied to the surfaces of the projecting cells, through which the air in the water is communicated to the cæca contained in them: these cæca I consider to be the respiratory organs.

In the leech, the water is received through the openings on the belly of the animal into the cells, or respiratory organs, and passes out by the same openings.

A knowledge of the mechanism employed for the pur-

poses of respiration in the sturgeon, and in the three first genera of animals mentioned in the present Paper, enables us to carry on a regular series of links, in gradation, from fishes in general to the myxine; every change in structure arising out of some peculiar habit of life, belonging to the animal in which it is met with.

In fishes, the gills are so formed, that the water forced from the mouth out at the gills, is applied to them in the most complete manner.

In the sturgeon, while swimming, respiration is carried on in the same manner, but when the sturgeon adheres to any substance by the mouth, which it has a power of doing by extending its lips, some other mode of respiration is required, and it is found that in the act of pushing out the mouth, apparently by the same means, the gill covers are drawn up so as to leave a large channel between them and the gills, through which the water is brought into the mouth and returned through the gills; there is also on the inside of the gill cover the same structure as on the side of the opposite gill, only to a smaller extent.

In the lamprey, the mouth is more constantly employed in laying hold of its prey and other substances, and therefore the respiratory organs are not connected with it, but situated near it.

In the myxine, which feeds upon the internal parts of its prey, and buries the head and part of its body in the flesh, the openings of the respiratory organs are removed sufficiently far from the head to admit of respiration going on, while the animal is so employed.

The respiratory organs in the two last genera mentioned

in this Paper, belong to a series of less complex structures, and perhaps, few animals can have a more simple mechanism than the leech.

EXPLANATION OF THE PLATES.

PLATE XI.

A view of a portion of the lamprey of the natural size, the organs of respiration exposed.

The mouth is laid open, exposing the teeth.

a. The tongue, on which there are teeth turned on one side.

b. The cavity of the mouth.

c. The fauces.

d. The tube between the bags containing gills.

e. Its termination in a loose edge at the orifice of the œsophagus.

f. A firm cartilage in the centre of the retractor muscles of the tongue.

gg. Two large salivary glands.

hh. The cavities containing a structure like gills laid open through their whole extent.

ii. The external orifices of these cavities.

kk. The internal orifices.

ll. The cartilages of the thorax.

m. The cartilaginous pericardium.

n. The termination of the œsophagus in the stomach.

Plate XII.

The respiratory organs in an animal from the South Seas, and in the myxine.

Fig. 1. These organs exposed in the animal from the South Seas.

a. The external nostril.

b. Internal nostril.

c. A tooth on the roof of the mouth.

dd. The tongue split, showing two rows of teeth on each side.

ee. The muscles of the tongue divided and turned aside.

ff. The œsophagus.

gg. The external openings into the respiratory organs.

hh. The internal ones,

ii. The organs themselves.

Fig. 2. One of the bags or organs laid open.

Fig. 3. The same parts in the myxine.

a. The external nostril.

b. The internal nostril.

c. A tooth in the roof of the mouth.

dd. The tongue split, showing two rows of teeth on each side.

ee. The muscles of the tongue turned aside.

ff. The œsophagus.

g. The stomach.

hh. The two external openings leading to the organs.

i. The opening leading to the œsophagus.

kk. The tubes leading to the organs.

ll. The internal openings.

mm. The organs themselves.

nn. The mucous glands.

PLATE XII.

The respiratory organs in the aphrodita aculeata and leech.

Fig. 1. Back view of the aphrodita aculeata.

Fig. 2. The respiratory organs laid bare by removing the skin and muscles on the back.

aa. The cells projecting into the cavity under the muscles of the back.

bb. The external openings leading into the cavity.

c. The gristly external [illegible] cartilaginous covering seen on the opposite side [illegible] removed.

d. The intestine.

ee. The lateral tubes [illegible] each side.

ff. The cæca, which [illegible] into the cells.

Fig. 3. The hirudo [illegible] drawn from behind, the stomach [illegible], exposing the respiratory organs, consisting of [illegible] transparent [illegible] in each of which the external [illegible] seen [illegible].

The [illegible] narrow [illegible], glands, and nerves distinctly seen.

aa. [illegible] cells.

bb. A large [illegible] on the same side.

cc. [illegible]

dd. Glandular [illegible] communicating with the testicles.

ee. The [illegible]

f. The [illegible].

g. The [illegible].

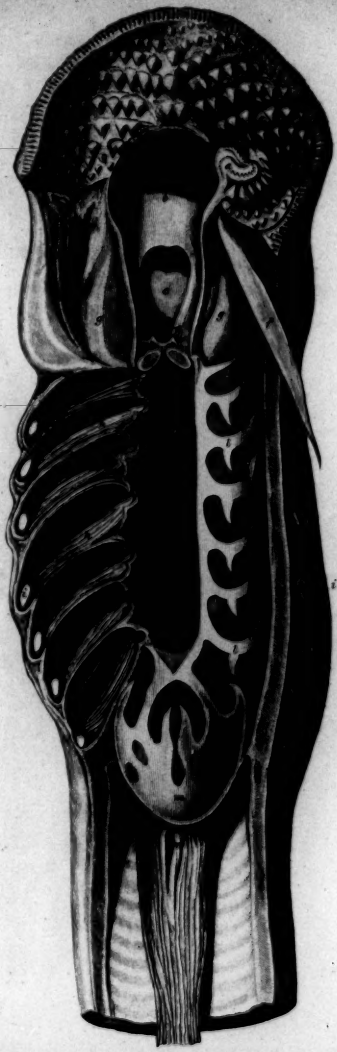


Fig. 1.

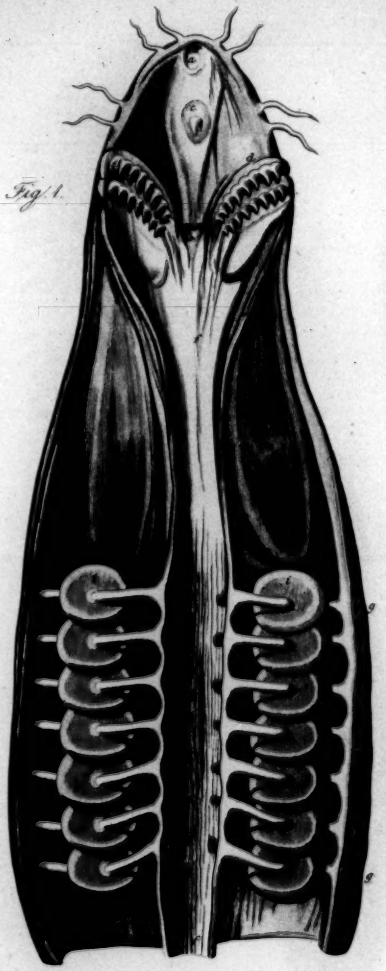


Fig. 2.

Fig. 3.

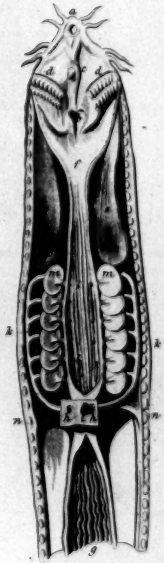


Fig. 1.

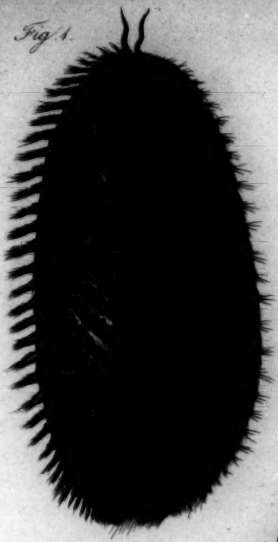


Fig. 2.

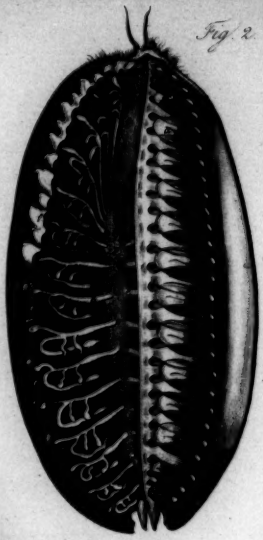
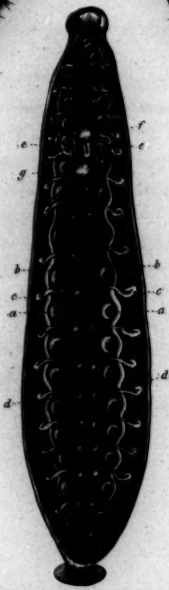


Fig. 3.



XVII. *On the mode of generation of the lamprey and myxine.* By Sir Everard Home, *Bart. V. P. R. S.*

Read June 15, 1815.

THE observations contained in the preceding paper upon the organs of respiration of the lamprey and myxine, led me to doubt the propriety of classing these animals among fishes, and as their ovaria differ in many respects from those of fishes in general, I became desirous of knowing the structure of the testicles in the male, to see how far they resembled those of fishes, or in how great a degree they differed from them.

That the lamprey was male and female nobody seemed to doubt, and yet with every opportunity apparently before me, I was unable to procure one male. Sir JOSEPH BANKS supplied me very largely with lampreys and lamperns, those that were full of ova were admitted to be females, and those which appeared to have no ova were said to be males; but all of them had ovaria, although in some the ova were extremely small, requiring a magnifying glass to distinguish them, in others they had been shed, leaving the cells in which they had been contained, and the openings by which they passed out very apparent. Being accidentally at Worcester, in which city, during the season, lampreys are caught in the river Severn in great abundance, and potted to be sent all over England, I made

inquiry of the person whose business it is to prepare them for being potted, what were the differences between the internal parts of the male and female; he said, the only difference was, the one had no ova, the other had, in all other respects they were alike. He had never seen a lamprey in which there was no part corresponding to what I called ovarium. This remark from a person whose whole employment during the breeding season was to take out their viscera, corresponded so entirely with my own observations, that I began to entertain the opinion that the lamprey has not distinct sexes, but is an hermaphrodite animal. This doubt of their being male and female, was started in the beginning of the breeding season, and my friend Dr. WILSON PHILIP of Worcester, supplied me with lampreys at regular intervals, till the ova were shed, that I might prosecute this inquiry. I found upon examination, that the two glandular bodies projecting into the belly, one on each side of the ovarium, which have been always supposed to be the kidneys, varied very much in size and appearance at the beginning and end of the season. When the ova are so small that the animal is reputed to be a male, these glandular bodies and the black substance upon which they lie appear to form one mass, and the duct upon the anterior part is thin and almost transparent, containing a fluid equally so, but in the end of May, when the ova increase in size, these glandular bodies become larger, more turgid, and have a distinct line of separation between them and the black substance behind; their structure is more developed, being evidently composed of tubuli running in a transverse direction, and the ducts leading from them are thicker in their coats and larger in size.

On the 5th of June, the ova were found to be of the full size, and a small transparent speck not before to be observed was seen in each; at this time the tubular structure had an increased breadth, and the duct going from it contained a ropy fluid, which when examined in the field of the microscope, was found to be composed of small globules in a transparent liquid.

On the 9th of June, neither the ova nor the tubular structure had undergone any change.

On the 11th, the ova were of the same size, but the slightest force detached them from the ovarium, the tubular structure had increased still more in size, the fluid in the ducts was thicker, more ropy, and when water was added to it in the field of the microscope, it coagulated, and what was before made up of globules, had now the appearance of flakes.

As these specimens had been kept two days, and had been carried 120 miles before they were examined, the appearance of the tubular part was seen under a disadvantage; but I was so fortunate on the 12th of June, as to receive from Sir Joseph Banks, the viscera of two lampreys caught in the river Thames, one of which had shed its ova, the other just ready to do so. In this last, the tubular structure from being in a more recent state was very distinct, and the difference in texture and appearance, between it and the black substance behind it, was more strongly marked. It is from this specimen that the annexed drawing was made, (Pl. XIV.) in which the black part is seen to have a reticulated texture. As it runs up as high as the heart, and may be said to lie principally behind the peritonæum, which is both the course and situation of the kidney in fishes, there can be no doubt that it performs the office of

that gland, while the tubular bodies which project into the cavity of the abdomen, and are increased to double their usual size at the time of shedding the eggs, must be considered as the testicles.

The ova in the lamprey do not pass out at an excretory duct as in fish, but drop from the cells in the ovarium in which they were formed into the cavity of the abdomen, and escape by two small apertures in the lower part of that cavity into a tube common to them and the semen, in which they are impregnated.

This mode of impregnation is much more economical than that employed in fish, as it explains the circumstance of the testicles being so small.

In the animal intermediate between the lamprey and myxine, and in the myxine, the organs of generation have the same structure as in the lamprey.

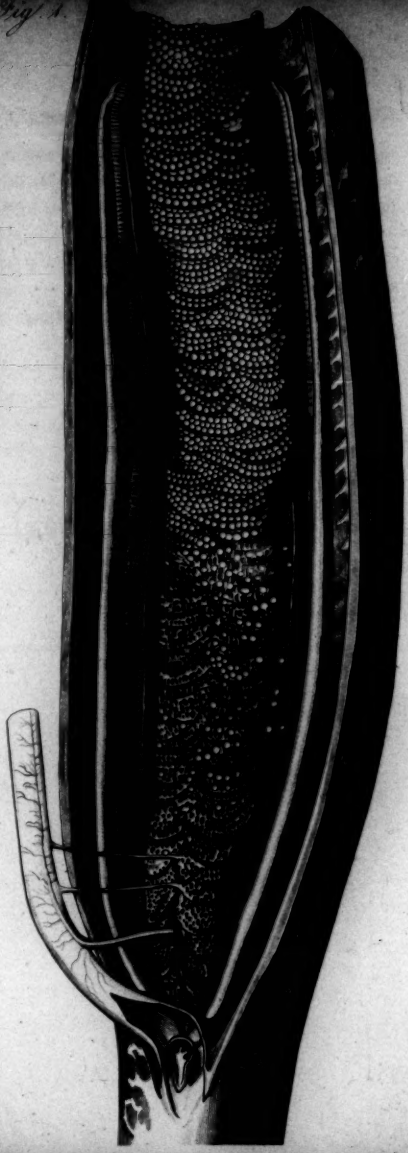
EXPLANATION OF PLATE XIV.

Fig. 1. A view of the ovarium of the natural size laid open, to show the ova in the cells from which they begin to be shed, some of them hanging loose in the cavity of the belly, others remaining in the cells of the ovarium in which they were formed.

Fig. 2. The testicle of the lamprey, with a glandular body projecting into the cavity of the abdomen, which I have explained to be the testicle. It is represented as placed in a transverse direction, which it is in the animal, composed of a reticular

Fig. 1.

Fig. 2.



texture, which extends higher than the testicle; this I consider to be the kidney. On the forepart of the testicle is the duct or vas deferens; this is laid open at its termination to show that it forms, with the opening into the belly, a common cavity just within the verge of the anus.

Fig. 2. A portion of the ovarium and testicle of the natural size, when the ova are beginning to enlarge.

XVIII. *On the multiplication of images, and the colours which accompany them in some specimens of calcareous spar.* By David Brewster, *LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P.R.S.*

Read June 8, 1815.

DEAR SIR,

THE multiplication of images exhibited in some specimens of Iceland spar, appears to have been first observed by Dr. JOHN ROBISON of Edinburgh, who showed the phenomenon to Mr. BENJAMIN MARTIN.* Having procured several specimens that had a similar property, Mr. MARTIN examined them with care, and published an account of his observations in his Essay on Iceland Crystal. The experiments of MARTIN were repeated by Mr. BROUGHAM, who concluded that the images were produced by fractures, parallel or nearly so to the sides of the rhomboid, and MALUS has more recently endeavoured to explain the phenomena, by the laws of extraordinary reflexion within doubly refracting crystals.†

All these philosophers agree in ascribing the multiplication of images to internal reflections, and they equally concur in regarding the colours of the images as the same with those of thin plates, and as produced by fissures, or fractures within the crystal.

* See BROUGHAM's "Further Experiments, &c. on Light." Phil. Trans. 1797.

† I am acquainted with MALUS's observations only from a short account of them given by Dr. YOUNG.

In this state of the subject, my attention was accidentally directed to it, in consequence of having received, from Sir GEORGE MACKENZIE, Bart., some very fine specimens of calcareous spar, which possessed the property of multiplying and colouring the images. I examined with attention the different planes in which the images were polarised; I found that small crystals detached from particular parts of the specimens, possessed the same properties, and I represented in figures the specimens which I employed, and the interrupting planes by which the colours were obviously produced. These results convinced me, that the interrupting plane was not a fissure or fracture; and I conjectured that the colours were analogous to those produced by the action of crystals upon polarised light.* By following out this conjecture, I have been led to the true cause of all the phenomena, and of other analogous facts; and have thus been enabled to communicate to any specimen of Iceland spar, the faculty of multiplying and colouring the images, in a manner so exactly similar to the real specimens, that no person can discern the least difference between the phenomena of the artificial, and those of the natural crystal. The results to which this explanation leads, will, I trust, be equally interesting to the mineralogist and to the natural philosopher.

SECT. I. *On the phenomena exhibited by particular specimens of Iceland spar.*

Let AEBFHDGC, (Pl. XV.) fig. 1. be a rhomboid of calcareous spar, and let the supposed fissure by which the coloured images are produced, be in the plane ABCD. When a pencil of

* Treatise on new Philosophical Instruments, &c. p. 339. and Pref. p. xii.

light is transmitted through the faces BCGE and AFHD, or through the faces BFHC and ADGE, the object from which it proceeds has the appearance represented in Fig. 2., consisting apparently of three images. The middle image A is white and is composed of two images A, *b* polarised in an opposite manner like the double image formed by common rhomboids of calcareous spar. The image B, which is highly and uniformly coloured, is polarised like A, and the image *a* is coloured in the same manner as B, and polarised in an opposite manner like the other image at *b*.

Let the rhomboid be now placed in such a position, that a horizontal pencil of light is incident upon the vertical face BCGE, and let the rhomboid be turned round a vertical axis, so that the pencil may be incident at various angles, the plane of incidence being always parallel to the horizon.

When the angle of incidence is about 10°, and the ray inclined towards EG, the two images B, *a* vanish, their angular distance being then about $3\frac{1}{4}°$, but at every other angle of incidence, these two images are visible. When the angle of incidence is gradually diminished, till it vanishes and then increases on the other side of the perpendicular, the reappearing images B, *a* separate from the middle image. The image B separates from it more rapidly, and increases in magnitude, in the same manner as when a pencil of light is incident obliquely *towards the refracting angle* of a prism, while the image *a* separates slowly from A, and contracts its dimensions, as when a pencil of light is incident obliquely *towards the base* of a prism. When the angle of incidence increases from the position where B and *a* vanish, these images approach to the middle image

A*b*; constantly varying their colours in such a manner, that the middle image A*b* has *a colour complementary* to that of the extreme images B and *a*.

If the angle of incidence is made to vary in a vertical plane, the extreme images separate from the middle image when the pencil is inclined to G*c*, but they approach it, and exhibit the complementary colours when the inclination of the pencil is towards BE. During these changes the colour of A is often complementary to that of *b*.

When the eye is placed in front of the face BCGE, so as to perceive the images reflected from the posterior surface AFHD, each of the images *a*, *b*, A, B is tripled in passing the plane ABCD, so that *nine* highly coloured images are distinctly visible.

In a very curious specimen of calcareous spar in the possession of Mr. MYLNE, there are two supposed fissures as shown at *afhd* and *ebcg*, fig. 7. These planes are equidistant from the obtuse angles E, F, and each of them produces three images which are never coloured, except when the incident ray is very oblique, and the two extreme images near the middle one. In this case, the colour of the middle image is distinctly complementary to that of the extreme images. When the light passes through the faces AEGD, BFHC in such a manner, that the eye receives part of the pencil acted upon by the plane *e b c g*, and part of the pencil acted upon by the plane *a f h d* each of the three images appears double, and in consequence of a third plane $\iota\beta\varkappa\gamma$, one of the extreme images is sometimes tripled, so that the eye may see at once *seven* images independent of the numerous images

which are formed by reflections from the sides of the rhomboid.

All the specimens of interrupted calcareous spar which I have examined, present a very remarkable and beautiful phenomenon which has not hitherto been observed. Let AEBF, Fig. 3. be a section of the rhomboid shown in Fig. 7, and BA the interrupting plane; a ray RS incident in a direction nearly parallel to BA will be refracted in the direction ST, and passing through the plane BA at T, will emerge in the direction VX. Another ray *rs* incident on the adjacent face BF, and parallel to SR, will be refracted in the direction *s*T, and suffering reflection at *T* in the direction TV will also emerge in the line VX. If the face BF is covered, an eye placed at X will perceive across the middle image A*b*, Fig. 2. a series of beautiful fringes concave towards B, Fig. 3. and separating a bright from a dark space which is towards B. The predominant colour in these fringes is yellow: their direction is perpendicular to the line joining the images *a*, *B*, Fig. 2, and they increase in breadth towards E, Fig. 3. fading away in pink and green fringes. If the face BE is now covered, an eye at X will perceive a series of fringes complementary to the first set, and having a bright blue for their predominant colour. They have the same curvature and direction as the first set, and separate a bright from a dark space which is towards E, but they are far superior to them in distinctness and splendour of colouring. By covering only a part of EB, we can see at the same time both the sets of fringes, the yellow stripes of the first set joining the blue stripes of the second. This junction of the fringes forms a very interesting pheno-

menon, and is the only perfect example in which the complementary fringes are seen at the same instant.* In the specimen shown in Fig. 7, one of the interrupting planes gives remarkably minute fringes, while the other forms them of a larger size.†

SECT. II. *On the position and character of the interrupting plane.*

In every specimen of calcareous spar which possesses the property of multiplying and colouring the images, there is a plane ABCD, Fig. 1. stretching across the crystal. This plane, which I shall call the *interrupting stratum*, has not the most remote likeness to a fissure or fracture, but resembles rather a thin vein or film cohering to the two prisms between which it is interposed. The lines AB, CD which form the termination of the stratum, are distinctly marked on the natural faces of the crystal, and form straight lines perpendicular to the shorter diagonal FE; and the rhomboid is divided by the interrupting stratum into two equal prisms ABCDGE, ABCDHF, having the angles ABE, BAF each equal to 39°.

If the plane ABCD is a fissure or a stratum of air, as has been supposed, it is demonstrable that a ray of light incident at an angle of 37° upon AB will suffer total reflection, and therefore no light will be transmitted through the rhomboid. So far, however, from this being the case, there is actually no angle of incidence at which total reflection takes place at the second surface AB, and consequently there is no physical

* An imperfect example of this I have given in the Phil. Trans. 1814, p. 227. Plate VIII. fig. 3.

† In the specimens represented in Plate XI. fig. 8 and 11, of my Treatise on New Philosophical Instruments, the fringes are very large.

breach of continuity between the two prisms. If the adjacent surfaces of the prisms were perfectly smooth, and flat, like plates of parallel glass, and if they were pressed together by a great force, total reflection would thus be prevented, and the light would pass through the fissure at any obliquity. But if total reflection were prevented in this manner, the pencil of light would experience no peculiar action in passing through the compressed surfaces, and therefore neither a multiplication of images, nor a decomposition of the pencil into colours could take place.

With the view of corroborating this reasoning, I endeavoured to separate the two prisms by force, but I found this quite impracticable. The crystal actually broke at another place, so that the two prisms cohere with great force, though this is the direction of one of the cleavages of calcareous spar, and though the supposed fissure extends to the very surface of the four faces of the rhomboid.

But admitting the existence of a fissure under these circumstances, it is demonstrable that it could not produce the phenomena described in the preceding section. I have examined several real fissures in calcareous spar, and though the colours of thin plates were seen by reflection, yet those formed by transmitted light, could scarcely be rendered visible.

The appearance on the middle image of colours complementary to those on the extreme images, is an irrefragable proof that they are not the colours of thin plates, in which one of the complementary tints must necessarily suffer reflection.

In order to remove all doubt respecting the effects of a fissure, I ground off the angles EG, FH, Fig. 1. till the

crystal was bounded by the artificial faces *a f h d*, *e b c g*, and having polished these faces, I transmitted a pencil of light through the interrupting plane. In this case there was neither a multiplication of images, nor a production of colour, and the same result was obtained though I caused the pencil to fall upon the interrupting stratum at the same angle at which it was incident when the images were multiplied and coloured. Hence, it is obvious, that if the colours were produced by a fissure, they ought still to have appeared even when the fissure was bounded by parallel plates of spar.

In the specimen which is shown in Fig. 7, we are presented with several curious facts relative to the interrupting plane. This specimen is intersected by three interrupting strata *afhd*, *ebcg*, *αβωγ*, the two first being equidistant from AB, and all of them having the same position relative to the axis of the rhomboid. The thickness of the interrupting strata is distinctly seen at *af* and *eb*, and is nearly $\frac{1}{200}$ dth of an inch, bounded by two distinct parallel lines. The upper surface *af* of the stratum *afhd* is on a level with the general surface AEBF, but the upper surface *e b* of the other stratum forms an angle of 141° with the plane *e* E *b* and is smooth and well polished. The lower surface *dh* is partly level with the general surface, and partly inclined at an angle of 141° to the plane *d H h*, and the surface *gc* is parallel to the inclined surface *eb*. The two strata *afhd*, *ebcg* have therefore a crystallized structure, and as they effervesce with nitric acid, we are entitled to consider them as flat rhomboidal veins of calcareous spar.

The stratum *ebcg*, which is shown separately in Fig. 8, is divided into portions by four or five veins *mn*, *op*, some of

which, such as *r* and *s*, are not complete. When any of these minute veins, such as *mn*, is seen through the faces *e*E G*g*, *b*E G *c*, Fig. 9, it is quadrupled, and appears, as in the figure, composed of four veins *m* 1, *m* 2, *m* 3, *m* 4, and *n* 1, *n* 2, *n* 3, *n* 4 diverging from *m* and *n*. By gently inclining the rhomboid, all these veins are brilliantly coloured, and, what is very singular, the colours of the middle veins *m* 2, *m* 3, *n* 2, *n* 3, are always complementary to the colours of the extreme veins *m* 1, *m* 4, *n* 1, *n* 4, exhibiting a much greater variety of hues than is seen in any other position of the crystal.

In order to observe the connection between the stratum *eb* and the contiguous prisms, I cut off part of the prism *e* E *b* and laid bare the surface of the stratum towards E. I then removed the stratum itself till I came to the adjacent surface of the prism, and in both cases I found the particles of the prisms adhering firmly to the stratum, though they were at such a distance from it that light incident obliquely suffered reflection.

From these experiments, we may safely conclude, that the interrupting stratum is not a fissure or fracture;—that it is a crystallized vein of calcareous spar, cohering firmly to the adjacent mass;—and that the multiplication of images and the colours which accompany them, are produced only when this vein is interposed between two solid prisms.

SECT. III. *On the cause of the multiplication of the images.*

If a ray of light RS is incident upon a rhomboid EBFA, Fig. 4, interrupted by a stratum of air AB, it will be divided by refraction into two pencils S*a*, S*b*. These pencils will be again refracted at the second surface *m n* into the directions

ac, *bd*, and falling upon the second prism at *c*, *d*, each of them will be again divided into two pencils, viz. *ac* into the pencils *ce*, *cf*; and *bd* into the pencils *dh*, *dg*. The pencils *cf*, *dg* emerging parallel to each other, will form a double image like A*b*, Fig. 2, while the other two pencils *ce*, *dh* will be inclined to these, and will form the single images *a*, B. The images will therefore be multiplied exactly as in Fig. 2, and if we calculate their angular distance, we shall find it coincident with the experimental results. This multiplication of the images may perhaps be more easily comprehended by supposing A, B, Fig. 2. to be two images formed by the first prism ABE, Fig. 4; then as the second prism ABF has an equal refracting angle, but placed in an opposite direction, it will refract the image B to *b*, and the image A to *a*, thus forming a double image in the middle, and a single image on each side of it polarised in the manner described in Sect. I.

In the preceding reasoning it is assumed, that there is an interruption in the structure of the rhomboid by which a subdivision of the rays takes place within the crystal. We shall now enquire how such an effect can be produced without a fissure. If we divide a rhomboid into two prisms ABE, ABF, and fill up the interval AB with a cement of the same or of a different refractive power from that of the calcareous spar, the ray RS will emerge in four pencils *ce*, *cf*, *dg*, *dh*, just as when AB was a stratum of air, and in so far as the multiplication of images is concerned, this artificial rhomboid will exhibit the precise phenomena described in Sect. I.

Hence it follows, that the multiplication of images arises from a subdivision of the two pencils at the first surface

of the second prism, and that the great angular distance of the images, which takes place even when the prisms are connected by a cement in perfect contact with each, is occasioned by the action of the doubly refracting force near the second surface of the first prism.

As all the phenomena of the natural crystal may be imitated by an artificial one, there can be no doubt that such changes actually take place within the crystal. It is interesting, however, to ascertain the principles on which these changes depend; and we are fortunately able not only to do this, but to apply the principles to the explanation of other phenomena exhibited by doubly refracting crystals.

It may be shown by various experiments, that the division of a beam of light into two pencils by double refraction, does not take place till the light has penetrated the first surface of the crystal, and suffered the ordinary refraction, while at the second surface the extraordinary refraction takes place before the emergence of the ray. The interposition, therefore, of a film AB of the same refractive power as the crystal, though it prevents the ray from suffering any ordinary refraction, still allows the extraordinary refraction to take place just as if the prisms were completely separated. For the same reason, the extraordinary refraction again takes place at the first surface of the second prism, and the two pencils are divided into four, as represented in Fig. 2.

Since the prisms ABE, ABF, Fig. 4, or the rhomboids which they contain, have their homologous sides parallel, the pencils *Sa*, *Sb* ought not to be divided into two by the second prism according to the observations of HUYGENS and NEWTON.

This, however, is true only when the pencil is incident at an angle of between 12° and 14°, as described in Sect. I.,* and we have already seen that in this case the images are reduced to two. In every other position of the incident ray, the pencils are subdivided by the second prism.

The division of the pencil into two parts after it has penetrated the first surface, or before it has emerged from the second surface of calcareous spar, enables us to explain the curious fact observed by Malus, relative to the light reflected from the interior surface of doubly refracting crystals. He discovered that the ray refracted ordinarily at the second surface was reflected at this surface in two pencils, one ordinary, and the other extraordinary; and that the ray refracted extraordinarily at the second surface, was also reflected in two pencils; so that there were four reflected rays, and only two emergent ones. These four rays returning to the first surface of the crystal, emerge in four parallel pencils, which form with this surface the same angle as the incident ray.

The cause of this singular fact will be understood from Fig. 5, where ABCD is a piece of calcareous spar, and mn, $\mu\nu$, the lines within the crystal at which the extraordinary refraction takes place. A ray of light RS will be divided into two pencils Sa, Sb, which will emerge in lines $a\alpha$, $b\beta$, parallel to RS. The reflected portions bd, ac will be subdivided at c and d, just as if they had been incident in the directions δb, $\varkappa a$, and will form four pencils ce, cf, dh, dg, which is the phenomenon observed by Malus. In order to show experimentally that the rays $a\alpha$, $b\beta$ are subdivided at c and d, when received upon the rhomboid in the directions δb, $\varkappa a$, cement

* I shall have occasion to consider this law in a subsequent Paper. I have stated the angle at between 12° and 14° as the pencil dh Fig. 4. vanishes at a less angle than cr.

a plate of glass GH, Fig. 6. upon the second surface CD, by means of a transparent cement EF. The rays *Sa*, *Sb* have now emerged completely from the calcareous spar, and being reflected from the glass plate GH, they again enter the crystal, and are subdivided as formerly at the line *ms*, into the four pencils *ce*, *cf*, *dg*, *dh*.*

In a specimen of calcareous spar examined by Mr. MARTIN, *twelve* images were seen, arranged in three rows. The middle row, consisting of *six*, was produced by two interrupting planes situated in the manner shown in Fig. 7, while the other two rows were formed by reflection from the sides of the rhomboid. Mr. BROUGHAM examined a specimen which afforded *six* images in some positions, besides other two, which, as this able writer justly remarks, were reflected from the sides of the specimen.

SECT. IV. *On the cause of the colours with which the images are affected.*

As there are some specimens of calcareous spar in which the multiplication of the images is not accompanied with the production of colours, the one phenomenon is not necessarily connected with the other, the multiplication of the images depending merely on the interruption in the regular structure of the mineral, and the colours upon the thickness and crystalline nature of the vein by which that interruption is produced.

We have already seen that the double image A*b* (Fig. 2.) is in general white, while *a*, and B, are affected with the same prismatic colour, and that when A*b* is coloured at particular angles of incidence, its colour is always complementary to

* MALUS believed that the fact of the subdivision of the reflected pencils was general; but there is obviously a particular angle of reflection at which four pencils are not formed.

that of *a* and B. These colours are therefore produced by the transmission of polarised light through the crystallized film AB, Fig. 4. The light is first polarised by the prism ABE: it is then separated into its complementary colours by the crystallized film AB, and this compound beam is analyzed by the second prism ABF. This arrangement, indeed, is the very same as that which I have described in a former paper,* as necessary for the exhibition of the complementary colours, the light being polarised by double refraction instead of by reflection, and being analyzed by a prism of calcareous spar, instead of a plate of agate.

In order to put this explanation to the test of direct experiment, I cut a rhomboid into two prisms ABE, ABF, Fig. 4, having equal refracting angles; and I interposed a thin plate of sulphate of lime between the two prisms. When the light was incident on the first surface EB at an angle of between 12° and 14°, so that the two images *a*, B, had vanished, I shifted the sulphate of lime till it ceased to depolarise the light, or restore the vanished images *a* and B. I then cemented it in this position to the two prisms, and thus obtained an artificial rhomboid, which imitated with the utmost exactness all the phenomena of the natural one. The extreme images *a*, B, became coloured, while the double image A *b* remained white, and the colours varied by varying the inclination of the plate to the incident rays. The images *a* and B approached to, and receded from, the middle image as in the natural crystal, and at particular incidences the middle image exhibited colours complementary to those of the extreme images, and of the very same kind with those in the natural rhomboid.

* Phil. Trans. for 1814, p. 210.

When the position of the sulphate of lime is changed, the depolarisation is increased, and the double image A*b* is no longer white, but always displays the colours complementary to those of *a* and B. In particular positions of the sulphate, the middle images become white at an oblique incidence.

In the specimen of calcareous spar represented in Fig. 7, the colours are by no means brilliant, and they appear only when the incident ray falls obliquely upon the rhomboid, with an inclination towards the base of the first prism. The reason of this will appear from Fig. 4. When the ray is incident obliquely towards the base of the first prism, as *r*S, it is refracted in the direction S *m*, and passes through the interrupting stratum AB nearly at its least thickness; whereas when it is incident obliquely in the direction *p* S, it is refracted into the line S*n*, and passes obliquely through the stratum at a thickness too great to produce the complementary colours. We are presented therefore with a method of determining rudely, the comparative thickness of the strata by which the colours are produced. In two very fine specimens, the colours are exhibited at almost every inclination, and they vanish when the inclination is near its maximum, and when the ray passes obliquely through the stratum.

Hence it follows, that the colours are produced by the transmission of polarised light through a crystallized vein, and that the phenomena change their character with the thickness of the vein.

We have already seen that the images may be multiplied without being coloured, but they cannot be coloured without being multiplied, as the separation of the oppositely polarised pencils is necessary to the production of the colours. This is

proved by grinding off the angles EG, FH. Fig. 1, so as to make the interrupting stratum parallel to the two faces of the crystal. There is in this case no multiplication of images, and no production of colour.

The complementary fringes described in Sect. I. are likewise produced by the transmission of polarised light through the interrupting stratum. When the ray RS, Fig. 3, is incident at various angles upon EB from the position *r* S, Fig. 4, to the position RS, Fig. 3, the refracted ray ST passes through the stratum AB at various thicknesses, and it therefore exhibits different colours corresponding to these thicknesses. When the ray has the position RS, the thickness of the film becomes so great, that the colours cease to be developed, and this limit of the production of colour is marked by parallel fringes gradually diminishing in breadth towards that limit. In like manner the ray *r s* being refracted in the direction *s* T, and falling upon AB at T, will pass through it at the same thickness as the ray RS does, and being reflected from the posterior surface of the stratum, will move in the direction TV, and emerge in the line XV. Fringes of the same character, but complementary to the former, will thus be produced by reflection, and from the equality of the angles STB, *s* TB and ATV, the fringes formed by transmission will be seen in the same direction XV as those seen by reflection. The reason is therefore manifest why the one set of fringes is seen by covering the face EB, and the other set by covering the face BF, and why both sets are visible when only part of EB is covered.

In order to show that these fringes are produced by the action of the crystallized film upon polarised light, I examined the phenomena in the following manner. As the double

image A *b*, Fig. 2, across which the fringes are visible, is formed by two equal and oppositely polarised pencils, it is necessary to the production of the fringes, that one of these pencils be either extinguished, or greatly diminished in its intensity. Now, if we cover BF, and examine with a prism of calcareous spar the pencil VX formed by the rays RS, we shall actually find that one of the images, *b* for example, Fig. 2, is very much fainter than the other, and therefore the fringes must be formed of the polarised light of A, being in this case very faint, owing to the admixture of the remaining light of *b*. If, on the contrary we cover EB, and examine the pencil XV formed by the rays *rs*, we shall find that the image *b*, Fig. 2, is almost wholly extinguished, and consequently the fringes formed by the polarised light of A are remarkably distinct, suffering no diminution of lustre from the admixture of oppositely polarised rays.

If the rays RS, *rs*, (Fig. 3.) are now polarised before their incidence upon the rhomboid, the fringes formed by *rs*, do not experience any change, in consequence of their being produced by unmixed polarised light; but the fringes formed by RS suffer a particular modification. When the plane of incidence EBFA forms an angle of 45° with the plane of polarisation, the fringes are extremely distinct and beautiful, and the same thing happens when the rhomboid is turned round 180°. In positions at right angles to these, no fringes whatever are visible. In the first of these positions, the pencil is not divided into two oppositely polarised pencils, whereas in the other position, it is divided into two oppositely polarised pencils of equal intensity.

If instead of polarising the incident rays RS, *rs*, we examine

the resulting pencil VX with a prism of calcareous spar, we shall find certain positions of the prism in which the fringes are invisible. In some positions they are finely displayed across the first image, and are not visible across the second; while in other positions the fringes across the first image vanish, and appear distinctly across the second. All these phenomena arise from the alternate evanescence of the images, which causes the fringes to be seen across the remaining image formed by light polarised in one plane.

The explanation which has now been given of the iridescent phenomena of calcareous spar, enables us to account for the origin of the colours peculiar to the agate, which I have described in a former Paper.* These colours always appear in veined agate, and are, undoubtedly, produced by the interposition of a vein between two equiangular prisms.

SECT. V. *Description of new instruments for exhibiting complementary colours.*

A simple instrument for exhibiting the opposite or complementary colours has long been a desideratum in the arts, as well as in the sciences. To painters, and to artists of almost every description, it is of very extensive use, while in many optical inquiries its advantages cannot be sufficiently appreciated.

The method of showing these colours which I have pointed out in a former Paper, consists in the separation of polarised light into two pencils, by the action of a crystallized plate, and in the subsequent analysis of the pencil, by a doubly refracting crystal. The simplest way of fitting up an instru-

* See Philosophical Transactions, 1813, p. 102, 103, and 1814. p. 197, 199.

ment upon this principle is shown in Fig. 10, where ABCD is a tube one or two inches long, S a piece of black glass forming an angle of 33° with the axis of the tube; *n* a convex eye glass placed next the eye; *o* an aperture of a circular or any other form in the focus of the lens *o*; *m* a flat piece of topaz or rock crystal* not much larger in diameter than the pupil of the eye, and cut in the proper direction from the crystal; BD a prism of nearly the same diameter formed out of rock crystal in the manner long ago described by M. ROCHON,† so as to produce the greatest separation of the images, or what is still simpler, a prism of calcareous spar having the refraction and dispersion as much as possible corrected by an opposite prism of balsam of Tolu or indurated Canada balsam. When the instrument is thus fitted up, the rays RS, polarised by reflection from the glass S, are arranged into their complementary colours by the crystallized plate *m*, and are afterwards separated into two distinct pencils by the double refraction of the prism BD. An eye, therefore, placed at *n*, will see two distinct images of the aperture *o*, and the colour of the one image will be complementary to that of the other. These images will exhibit alternate variations of colour by turning round either the tube, or the polarising plane S. If

* A thin film of sulphate of lime is much better than any other mineral, as it requires no trouble to prepare it. Topaz is preferable to rock crystal, as the latter very often gives false tints, from a want of uniformity of structure. Mr. SANDERSON, an ingenious lapidary in Edinburgh, has cut about twelve plates of rock crystal parallel to the axis of the pyramid, and observed, that all of them were filled with veins and imperfections radiating from the axis. In a large pyramid about 2½ inches in diameter, these radiations are arranged in the form of a cross, forming angles of 60° and 120°, and they terminate on the faces of the pyramid.

† *Journal de Physique*, 1801. *Mémoire sur le Micrometre de Cristal de Roche*. Paris, 1807.

the two images overlap, the parts that overlap will be white, in consequence of the combination of the two opposite colours. The object of using the lens *n*, is to shorten the tube, but if we remove the eye glass, the aperture *o*, may be made of any size, and placed at any distance from the eye.

I have been induced to give this particular account of the preceding instrument, as another instrument upon the same principle, but of a most unphilosophical construction, has recently been exhibited in Edinburgh as a new invention, without any mention having been made either of M. ARAGO or myself, who separately discovered the property of polarised light on which it depends, or of M. ROCHON who invented the eye-piece of the instrument. It consists of a tube from twelve to twenty inches in length for the purpose of producing a sufficient separation of the images, and a large object glass of rock crystal is placed at the very end of the tube, although a small piece $\frac{1}{8}$th of the size would have answered much better, and admitted of a larger aperture if placed near the eye. In order to polarise the light, the operator carries a large square plate of japanned metal, and places it as near the polarising angle as he can. The instrument is then directed to this plate, and exhibits two overlapping images affected with the complementary colours.

The investigation in the preceding pages, furnishes us with a principle for constructing a new *antichromatic instrument*, far superior to any of the preceding, and so very simple, that any person can make it for himself. It is represented in Fig. 11, where MNOP is a tube about two inches long attached to a ball and socket. The end MO of the tube carries an aperture of any form, and the ball CD contains two

prisms of calcareous spar separated by a film of sulphate of lime so placed that each pair of the four images is tinged with the complementary colours as described in Sect. IV. A lens L is cemented either upon the anterior or posterior surface of the compound prism, or may be kept separate from the prism at L, but whatever be its position, it must always enable the eye at E to see the aperture with perfect distinctness, and the focal length of the lens must be so adapted to the magnitude of the aperture, that the images of it can be sufficiently separated by the universal motion of the ball CD. The interior of the tube being covered with a black pigment, the instrument is ready for use. If we direct it to the sky, or to any luminous object, four brilliantly coloured images of the aperture will be distinctly seen, the colour of the two middle images being complementary to that of the two extreme images. By moving the ball in the socket, the colours will constantly change, and the images will sometimes overlap, and sometimes separate, exhibiting the finest variety of hues, and pleasing the eye by their combinations, and by the soft harmony of their contrasts.*

In the instrument where it is necessary to polarise the light by black glass, or japanned metal, there is no less than $\frac{12}{13}$ths of the incident pencil lost by reflection, while in the preceding instrument the light lost by transmission is very small. From this cause, the brightness of the colours is incomparably greater, and they may even be distinctly seen in candle light, by directing the aperture to a piece of white paper held near the candle.

* The phenomena will admit of many beautiful variations, by using several films of sulphate of lime, having their axes variously inclined to one another.

I shall now conclude this letter with the description of another instrument which I have found of great advantage in carrying on very delicate experiments on the polarisation of light. In comparing the quantities of light polarised in the plane of reflection by different metals, we derive very little aid from examining the partial evanescence of one of the images. The intensity of the complementary colours is a much more delicate measure of the portion that has received this character. The method of doing this is shown in Fig. 12, where ABCD is a tube about eight or nine inches long supported upon a stand. An equal and unbroken plate of *sulphate of lime* which gives an uniform tint in every part of its surface, is cemented with Canada balsam between two plates of parallel glass, and is placed at the end AB of the tube exposing two circular apertures *m*, *n*, to the incident rays. At the other end of the tube is a piece of black glass *op*, inclined at an angle of 33° to the axis of the tube, and having a motion of rotation round that axis. When polarised light is transmitted through the apertures *m*, *n*, and reflected from the surface *op* to the eye, these apertures will appear equally coloured in every position of *op*, the colour in one quadrant of its circular motion being complementary to that in the adjacent quadrants. If we now wish to compare the quantity of polarised light in a pencil reflected at an angle of 80° from *silver*, with the quantity polarised at the same angle by *steel*, we have only to transmit the one pencil through *m*, and the other through *n*, and the intensity of the colours will show which of the two contains the greatest quantity of polarised light. Or if we diminish the inclination of the steel surface

till the colours of the two apertures are equally intense, we obtain the angles of incidence at which *steel* and *silver* polarise equal quantities of light in the plane of reflection. By using two plates of black glass having a variable inclination to the axis of the tube, we may allow the light to fall at equal angles upon the two metals, and thus ascertain the different inclinations of the plates of glass at which the two apertures exhibit the same intensity of colour.

I have the honour to be, &c.

DAVID BREWSTER.

Edinburgh, May 1, 1815.

To the Right Hon. Sir JOSEPH BANKS, Bart. G. C. B. P. R. S. &c. &c. &c.

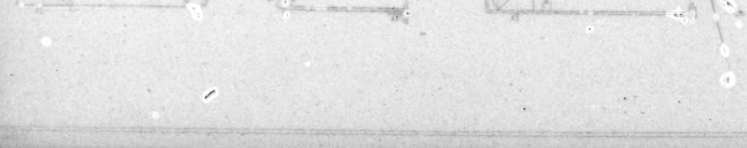


Fig. 1.

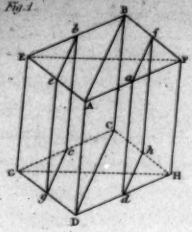


Fig. 3.

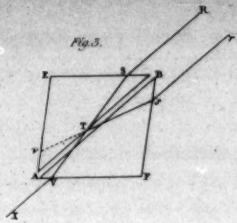


Fig. 4.

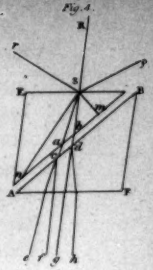


Fig. 2.

Fig. 5.

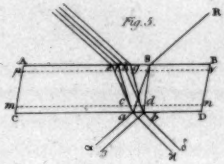


Fig. 6.

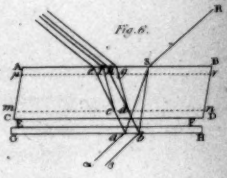


Fig. 7.

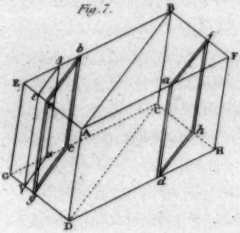


Fig. 8.

Fig. 9.

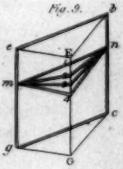


Fig. 10.

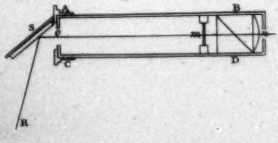


Fig. 11.

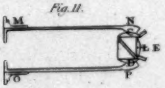
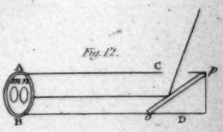


Fig. 12.



XIX. *A series of observations of the satellites of the Georgian planet, including a passage through the node of their orbits; with an introductory account of the telescopic apparatus that has been used on this occasion; and a final exposition of some calculated particulars deduced from the observations.* By William Herschel, *LL.D. F.R.S.*

Read June 8, 1815.

THE observations of the satellites of the Georgian planet, of which an account is given in this paper, are of such a nature that, in order to judge of them properly, and to make them useful to those who would continue them, it will be necessary to enter into some particulars relating to the telescopic powers required for critically viewing such difficult objects.

The great distance of the Georgian planet renders an attempt to investigate the movements of its satellites a very arduous undertaking; for their light, having to traverse a space of such vast extent before it can reach us, is so enfeebled, and their apparent diameter so diminished, that an instrument, to be prepared for viewing them, must be armed with the double power of magnifying and of penetrating into space.

With regard to the first of these requisites, I have already shown in a former Paper,* that the magnifying power of my ten feet telescope, when no uncommon degree of light is

* Phil. Trans. for 1805, page 31.

wanting, is fully equal to what may be required to view extremely small objects; but this branch of the properties of optical instruments seems not to be generally understood: the question how much a telescope magnifies, admits of various answers. To resolve it properly, we ought in all circumstances to consider how far the magnifying power of a telescope is supported by an adequate quantity of light; as without it, even the highest power and distinctness cannot be *efficient*. The question therefore ought to be limited to an inquiry into the extent of what may be called the *effective* magnifying power? It will however be found, that even then, the quantity of this power cannot be positively assigned. For if a card containing engraved letters of a certain size be put up at a given distance, the effective power of a telescope directed to it, will be that wherewith we can read these letters with the greatest facility; but if either the size of the letters, or their distance from the telescope, be changed, the quantity of this power will no longer remain the same.

An obvious consequence of this consideration is, that the effective power of telescopes has a considerable range of extent, and can only be assigned when the object to be viewed is given; and that in this determination two circumstances are concerned, which require a separate investigation; and this is abundantly confirmed when a ten feet reflector, such as has been mentioned, is directed to the Georgian planet; for with none of its highest powers can we possibly ascertain even the existence of the satellites.

Since, then, it is absolutely necessary that the power of magnifying should be accompanied with a sufficient quantity of light, to reach the satellites of this remote planet, it may

be useful to cast an eye upon the action of a power which is become so essential. Its advantages and its inconveniences must equally be objects of consideration.

A very material inconvenience is that mirrors, which must be large in order to grasp much light, must also be of a great focal length; and that in consequence of this, we must submit to be incumbered with a large apparatus, which will require an assistant at the clock and writing desk, and also an additional person to work the necessary movements. The machinery of my twenty feet telescope is however so complete, that I have been able to take up the planet at an early hour in the evening, and to continue the observations of its own motion, together with that of its satellites, for seven, eight, or nine hours successively.

The forty feet telescope having more light than the twenty feet, it ought to be explained why I have not always used it in these observations. Of two reasons that may be assigned, the first relates to the apparatus and the nature of the instrument. The preparations for observing with it take up much time, which in fine astronomical nights is too precious to be wasted in mechanical arrangements. The temperature of the air for observations that must not be interrupted, is often too changeable to use an instrument that will not easily accommodate itself to the change: and since this telescope, besides the assistant at the clock and writing desk, requires moreover the attendance of two workmen to execute the necessary movements, it cannot be convenient to have every thing prepared for occasional lucid intervals between flying clouds that may chance to occur; whereas in less than ten

minutes, the twenty feet telescope may be properly adjusted and directed so as to have the planet in the field of view.

In the next place I have to mention, that it has constantly been a rule with me, not to observe with a larger instrument, when a smaller would answer the intended purpose. To use a manageable apparatus saves not only time and trouble, but what is of greater consequence, a smaller instrument may comparatively be carried to a more perfect degree of action than a larger one; because a mirror of less weight and diameter may be composed of a metal which will reflect more light than that of a larger one; it will also accommodate itself sooner to a change of temperature; and when it contracts tarnish, it may with less trouble be repolished; to which may be added, that having two mirrors for the twenty feet always ready, my observations could never be interrupted by accidents which often happen to large mirrors, such as greatly injure, or even destroy their polish.

The quantity of light reflected by the mirror of a twenty feet telescope of my construction being known, and the satellites of the Georgian planet being the objects to be viewed, I may now examine the combined powers of this instrument, and assign the limits to which they may be stretched. It will however be proper first, to point out from experience some of the advantages that may be taken, if not to increase, at least not to obstruct, the penetrating power, by the full effect of which the magnifying power is to be supported.

The first precaution I ought to give is, that in these delicate observations, no double eye glass should be used, as it cannot be prudent to permit the waste of light at four surfaces,

when two will collect the rays to their proper focus. The hole through which they pass in coming to the eye should be much larger than the diameter of the optic pencils, and considerably nearer the glass than their focus; for the eye ought on no account to come into contact with the eye piece; and a little practice will soon enable the observer to keep his eye in the required situation. It is hardly necessary to add, that no hand should touch the eye piece.

With regard to the eye glasses, when merely the object of saving light is considered, I can say from experience, that concaves have greatly the advantage of convexes; and that they give also a much more distinct image than convex glasses.

This fact I established by repeated experiments about the year 1776, with a set of concave eye glasses I had prepared for the purpose, and which are still in my possession. The glasses, both double and plano-concaves, were alternately tried with convex lenses of an equal focus, and the result, for brightness and distinctness, was decidedly in favour of the concaves.

For the cause of the superior brightness and sharpness of the image which is given by these glasses, we must probably look to the circumstance of their not permitting the reflected rays to come to a focus.

Perhaps a certain mechanical effect, considerably injurious to clearness and distinctness, takes place at the focal crossing of the rays, which is admitted in convex lenses.*

* About the same time that the experiments on concave eye glasses were made, I tried also to investigate the cause of the inferiority of the convex ones; and it occurred to me, that an experiment might be made to ascertain whether the rays of light in

I have occasionally availed myself of the light of concave eye glasses, but a great objection against their constant use is, that none of the customary micrometers can be applied to them, since they do not permit the rays to form a focal image. Their very small field of view is also a considerable imperfection; in observations, however, that do not require a very extensive field, such as double stars or the satellites of Saturn and the Georgian planet, this inconvenience is not so material.*

As I have already shown that the *effective* power of a telescope arises from the combination of its magnifying and space penetrating powers; and have also proved that the effect of their union, when they are differently combined, must have a considerable range, it will now be easy to point out the extent of this range in the telescope by which the following observations have been made.

The magnifying power by which the satellites of the planet were discovered was only 157; but this power, which has been

crossing, jostled against each other, or were turned aside from their right lined course by inflections or deflections. With a view to this, I directed a 10 feet telescope to some finely engraved letters put up at a convenient distance. A convex eye glass was fixed to a skeleton apparatus, which left the focal point freely exposed. A concave mirror was placed so as to throw the focus of the sun's rays upon the focal image of the telescope, where, meeting with no intercepting body, they would freely pass through it at right angles. Then a screen being placed to keep off the solar rays, I fixed my attention upon the letters viewed in the telescope, and the screen being alternately withdrawn and replaced, I could perceive no sensible alteration in the brightness or distinctness of the letters. Hence I surmised, that the rays of light did not sensibly jostle in an instantaneous right angled passage, but that possibly they might suffer inflections or deflections in their crossing at the focal point on account of their being longer in collateral proximity.

* See Phil. Trans. for 1794, p. 58.

constantly used in my sweeps of the heavens, and was found to be very *effective* for the discovery of faint nebulæ and minute clusters of stars, is hardly sufficient to show the satellites steadily; for, unless every thing is favourable, their faint scintillation will only be perceived by interrupted glimpses.

The magnifiers 300, 460, 600 and 800, it will appear by the following observations, have gradually been found to be more effective on the objects on which they were used; according to the clearness of the air, the altitude of the planet, the absence of the moon, the high polish of the mirror, and other circumstances: on particular occasions, when doubtful points were to be resolved, even 1200 has been most effective. The higher magnifiers 2400, 3600 and 7200 have also been used to scrutinize the closest neighbourhood of the planet, in order to discover additional satellites; but, from the appearance of the known ones, which began to be nebulous, I concluded that these powers were not distinct enough to be used on this occasion.

As the following observations are given for the purpose of enabling astronomers to calculate the elements of the orbits and motions of the satellites with mathematical precision, I have endeavoured to save them some labour by giving a clear statement of the general outlines of them; and that some judgment may be formed of their accuracy, which I hope will be found considerable, a short detail of the method I have pursued will be necessary.

For ascertaining the position of the satellites from which their periodical revolutions were determined, three different methods have been used.

Coarse estimations were made when they seemed to be

sufficient to keep the satellites in view, by way of ascertaining their identity; for unless they were followed in their course and known to be satellites, it would have been endless to measure either the distance or position of every small star that might have the appearance of one; and as the opportunities for taking measures, which require a very clear and undisturbed atmosphere, were scarce, and often interrupted by cloudy or moonlight nights, the identity of the satellites would have been doubtful if their position had not been attended to, when seen in unfavourable circumstances. When no other stars interfered, it was often sufficient barely to mention the quadrant in which they were seen, by recording that such a satellite was np, nf, sf, or sp; or if necessary, some rather more determined account, such as 40 or 50 degrees np, sf, &c.

As a check upon the description of the situations, a figure was always added to represent the planet, its satellites and the neighbouring stars as they appeared in the telescope. Very often indeed the configuration itself was deemed to be sufficient to point out the situation of the satellites, which by way of distinction were marked by numbered points; 1 and 2 being used to distinguish the known satellites; 3, 4, 5, &c. those that might possibly be other suspected, but not ascertained ones. Stars instead of points, were marked by asterisks.

More careful estimations were made with a power not less than 300, and a wire in the focus of the eye glass, to ascertain the parallel; they are capable of considerable accuracy in situations that are only a few degrees north or south preceding or following, and also when the position of a satellite is nearly 90 degrees north or south of the planet.

Measures taken with the micrometer may always be supposed to be accurate, unless they are marked as being affected by some circumstances existing at the time they were taken: when these are favourable, they can hardly be liable to any great error.

The calculations which I have given with the observations, will show the appropriate confidence each of these three methods of obtaining the positions of the satellites may separately deserve.

A much greater difficulty attaches to taking measures of distances than to those of angular positions: when the latter are taken, we have the position of the satellite in view all the time the planet passes along the parallel; and, although the moment of ascertaining the angle is only that in which the planet is in the centre of the wires, yet a constant attention to the motion of the two bodies will sufficiently enable us to perceive any excess or defect in the parallelism between their situation and that of the adjustable wire, whereas in measures of distances, the telescope must be kept in motion to retain the two bodies in their contact with the two wires, which disturbance considerably affects the delicacy of vision, and moreover requires a divided attention, as the passage of each body over its respective wire must be viewed. The only exception is, when the satellite is at 90 degrees, in which case the distance of the two bodies may indeed be measured with great accuracy.

The lucid point micrometer which has been tried is subject to the same difficulties;* its application to my construction of the 20 feet telescope, with regard to situation, is very conve-

* For a description of this micrometer, see Phil. Trans. for 1782, page 163.

nient. When the apparatus was preparing, I found that handles, 20 feet long, would be very cumbersome, and attempted to try the micrometer with the assistance of a person to arrange the points; but, when engaged in the first measure, I found that unless I had myself the command of the motions, a perfect adjustment could not be obtained; or would at least take up so much time as would bring on an alteration in the telescopic motions, not consistent with perfect vision. This micrometer has, however, the peculiar advantage that it may be used with a concave eye glass.

When a satellite is either directly preceding or following the planet, its distance may be measured by the difference of the time of their passing the meridional wire. This method, which has also been tried, is however not sufficiently delicate for very small intervals, and is moreover of little use, on account of the very limited situations.

The following observations on the satellites of the Georgian planet are given in the order of time they were made. They contain every thing that relates to the appearance and motion not only of the two principal large satellites, that are plainly within the reach of a 20 feet telescope of my construction, but also the more difficult researches that have been pursued for detecting additional satellites. That such there are I can have no doubt; but to determine their number and situation will probably require an increase of the illuminating power, such as I was in hopes, when I published my announce of their existence, would have been used by other astronomers, in pursuit of the subject pointed out to them; a 25 feet reflector which is mentioned in the observations, may probably be sufficient for the purpose.

To facilitate calculation, the observations are all given in mean time, and after each of them is added a theoretical exposition of the place of the satellites, which I have called an identification, and is denoted by the sign ‡; the great use of which will be to point out the validity of each observation, by comparing the observed places with the theoretical ones. The method of identification, which will be described hereafter, by giving not only the angle of position at which a satellite ought to have been seen, but also its proportional distance in 600 dth part of the radius of its orbit, is of great consequence when the orbits of the satellites are much contracted. These distances indeed become at last the only criterion by which we may know the satellites, for the angle of position, when the planet is near the node of the orbits, admits of so little change that it ceases to be a direction for identifying them.

The same distance will also give us the total value of the measure of any distances taken by the micrometer, so far at least as to show which of them may be the most proper to be chosen for a more rigorous investigation.

An identification of supposed satellites cannot be made by calculation; but the observations of following and also of preceding nights, accompanied by accurate configurations, may ascertain whether the object in question be of a sidereal or planetary nature. For if by the removal of the planet a supposed satellite be left in its former place, it is decidedly a star; whereas a well ascertained absence from the observed place will make its planetary nature highly probable. Then also, if a configuration and description of every small star, that is situated in, and very near the path of the planet, has been previously made, and additional stars are afterwards

found to be near the planet, which cannot be accounted for, it becomes again probable that such questionable objects are of a planetary nature. And this being a kind of identification, I have added it after the calculated one, to every observation of doubtful objects, except where a supposed satellite is pointed out which there is reason to believe may be a real one; for in that case, the observations relating to the object in question, are given in their regular order.

It will not be necessary to give the configurations that were made at the time of observation; they generally contained the planet, its satellites, and some of the neighbouring stars, especially those that were in the path of the planet's motion; nor will it be necessary to mention lines and descriptions of situations of stars pointed out by letters affixed to them, as the observations are generally so redundant, that I found it highly necessary to compress them.

Observations of the satellites of the Georgian planet, accompanied by a theoretical determination of their situation, whereby their identity may be ascertained.

1787, January $11^d\ 12^h\ 13^m$. There is a supposed first satellite about 42 or 43 degrees south following the planet; and a second about 45 degrees north preceding. A third supposed satellite is south following the planet.

‡ By the identifying method, it appears that a real satellite, called the first, was visible at the time mentioned about $45\frac{1}{2}$ degrees south following; which agrees with the estimation of the angle of its situation, and also with a configuration of the stars and planet, drawn at the time of observation. By

the same method it appears that a real satellite called the second, was visible about 65 degrees north preceding the planet; which situation agrees with the configuration and also sufficiently well with a coarse estimation, which, as there was no wire for the parallel in the focus of the eye glass, could not be accurate. The supposed third satellite, by subsequent observations, was found to be a star.

1787, January 12. The first and second satellites are not to be seen in the place where I saw them last night. The supposed third is left where it was. I can see no small star near the planet, but the evening is not sufficiently clear.

1787, January 14, $12^h\ 3'$. There are again three supposed satellites; I have marked them 1st, 2d, and 3d, without any particular reason for that order.

‡ The first satellite was 88° nf; which agrees with the situation of that which in the configuration was marked 1st. The second satellite was 29¼ sp, and agrees with that which in the configuration was marked 3d. In the configuration the numbers are placed according to their distances from the planet, and that which is marked 2d was found to be a star remaining in its place.

1787, January 17, $11^h\ 51'$. There are now again three supposed satellites. The first is south preceding the planet, and makes a right angle with the 2d and 3d. The second is at the angular point and is south of the planet but a little preceding. The 3d is north following the planet. I have also added a 4th and 5th. The night is very fine and my telescope bears a high power.

‡ The first was 34° sp, which agrees with the configuration. The second was south of the planet but a little following,

namely 80¾° sf; which agrees sufficiently well with an estimation made without a direction for the parallel. The supposed 3d, 4th, and 5th, by next night's observation, remained in their places as small stars.

1787, January 18, 11ʰ 45′. There are two supposed satellites; the first is directly south of the planet; the second is about 45 degrees south following, and a little farther from the planet than the first. With 480 the first is about 4 diameters of the planet distant from it; the second is about 4½ or 5 diameters from the planet; the first is from the second about 2½ diameters of the planet. There is no small star in the path of the planet that might be taken for a satellite to morrow.

‡ The first was 76⅔ sf. The second was 59⅓ sf; both these positions agree sufficiently well with the delineated configuration.

1787, January 24, 11ʰ 23′. The first and second satellites of January 18, are no longer in the place in which they were that night. There are two satellites; the first is about 45″ np the planet; the second is about 80° np; it is brighter than the first. I had a glimpse of a 3d and 4th.

‡ The first satellite was 49° np; the second was 75½ np, which agrees well with the estimations and with the configuration. The observations of the third and fourth were lost, the planet not being seen again till eight days after, when it would have taken up too much time to look for them.

1787, February 4, 6ʰ 21′. The first satellite is about 80° sp; the second is about 30° nf. There is too much day-light to see the satellites well. A third supposed satellite is south preceding the first; it is extremely small. There is but one

single small star in the path of the planet which to morrow night may be taken for a satellite.

‡ The first satellite was 50° sp; the second was 40° nf. This differs considerably from the estimations, probably owing to the remaining day-light; the satellites however could not be mistaken, as there were no other stars near the planet.

1787, February 5, 9^h 8′. Both satellites are certainly absent from the place where I saw them last night. The first is about 85° sf; the second satellite (miscalled a small star) is by the configuration at a great angle nf. The small star in the path of the planet observed last night remains in its place.

‡ The first satellite was 89° sf; the second was 69° nf.

1787, February 6. I compared the configurations of January 11, 14, 17, 18, 24, February 4 and 5 together, and found that, admitting one of the satellites to make a revolution round the planet in about $8\frac{1}{4}$ days, and supposing its orbit to be very open to the visual ray, there was always one that would answer to a projection made on that scale.

1787, February 7, 6^h 54′. A satellite (miscalled the third) is a few degrees south following 6^h 30′, another (miscalled the first) is about 65° np. A small star (miscalled the second satellite) is about 60° sp.

‡ The first satellite was $11\frac{1}{4}$ sf. The second was $68\frac{1}{3}$ np. Two days after, the miscalled second, was seen remaining in its observed place. In the course of about nine hours of observation, I saw the planet accompanied by its two satellites, very evidently moving together in the path of the planet.

1787, February 9, 10^h 39′. Both satellites are gone from the place where I saw them the 7th of February. The first

satellite (miscalled the second) is directly north of the planet; the second (miscalled the first) is a few degrees np.

‡ The first was $81\frac{1}{2}°$ nf; the second 11° np.

1787, February 10, 8^h 57′. The first satellite is about 58° np. 8^h 38′. The second satellite is about 20° sp; a supposed third is about 45° sf. In a little more than four hours, I saw the satellites go on with the planet, and also in their orbits.

‡ The first satellite was $67\frac{1}{4}°$ np; the second was 20° sp. The supposed third was lost, no subsequent observation having been made of it. Before I began observing, I had delineated their places on paper, on a supposition that one of them moved at the rate of $8\frac{1}{4}$, the other at that of $13\frac{1}{3}$ days the revolution.

1787, February 11, 13^h 28′. Between flying clouds I saw the second satellite.

‡ The satellite was $57\frac{1}{2}°$ sp, which agrees with the configuration.

1787, February 13, 10^h 0′. The first satellite, with 300, is about 75 or 80° sp; its distance from the planet is about $\frac{3}{4}$ of a minute. The second is about 85° sf; its distance is one full minute; the estimations are by the field of view of the sweeping piece. Third, fourth, and fifth supposed satellites were marked.

‡ The first satellite was $68\frac{1}{4}°$ sp; its distance was 553, the radius of its orbit being 600. The second was $80\frac{1}{4}°$ sf; its distance was 599, the radius of its orbit being also 600. The third, fourth, and fifth supposed satellites proved to be stars. No great accuracy can be expected from the estimated distances given in the observation, the field of the eye piece, which took in 15 minutes, being much too large for the purpose.

1787, February 16, 9^h 38′. The two satellites are in the places where I had drawn them on paper. With a power of 300, and a wire for the parallel in the focus of the eye glass, the first satellite is, by very accurate estimation, about 5 degrees north following; at the same time, and with the same power and accuracy, the second is about 3 degrees south following. A third supposed satellite is pointed out.

‡ The position being so near the parallel, and by calculation also near the conjugate axis of the elliptical projection of the orbits, and therefore less liable to an error arising from the application of correction, have been fixed upon as standards for the calculation of the periodical revolutions of the satellites. The supposed third was next evening observed to remain in its place.

1787, February 17, 7^h 58′. I tried to measure the distance of the second satellite from the planet by a lamp micrometer. The lucid points were 246,4 inches from the eye, and when they were 14,4 inches from each other, I found that the adjustment of the distance, and angle of position could not be made to my satisfaction by an assistant, and gave up the measure. The magnifying power being 137, the opening of the points gives the angular distance 1′ 17″; but the measure when given up was still much too large.

‡ The satellite was $28\frac{1}{2}^\circ$ nf, and the distance 505.

1787, February 19, 7^h 55′. Having delineated the situation of the satellites on paper, I found them in the expected situation. A third and fourth were added in the configuration.

‡ The first satellite was 58° np; the second was 82° nf. The supposed third and fourth proved to be stars.

1787, February 22, 7^h 14′. By the configuration the first

satellite is at a considerable angle sp; the second is at a moderate angle np. Third, fourth, and fifth satellites were noticed.

‡ The first was 76° sp; the second was 31° np. A long interval happening to prevent subsequent observations, the supposed satellites were lost.

1787, March 5, 7^h 14′. The first satellite is about 6° sf. 7^h 17′, the second is about 87° nf; a third is about 40° nf.

‡ The first satellite was 90¼° sf; the second was 87° np; the third proved to be a star. The planet was only observed about 3 or 4′, and it does not appear that great accuracy in the estimations was attempted.

1787, March 7, 7^h 19′. The first satellite is 82° np. 7^h 18′ the second is about 30° np. Very coarsely estimated. A third is about 6° nf; it seems to have a fourth close to it. Having some doubts about the fourth, I viewed it with 600 and 800; I saw it also well with 1200, and had a glimpse of it with 2400. These high magnifiers require a fine apparatus for adjusting the focus.

‡ The first satellite was 78¾° nf; the second was 41° np; the third and fourth proved to be stars.

1787, March 8, 8^h 52′. Both satellites were seen for a few minutes.

‡ The first was 70° np; the second was 9½° np.

1787, March 11. I found that some friends who came to view the satellites saw them best with 480, when the planet was drawn to the margin just out of the field.

1787, March 15, 8^h 7′. The first satellite is about 48° nf; the second is 5° sf. The second satellite being so nearly following the planet, I tried to measure its distance by sidereal

time. Of eight transits, four gave 3″; three gave 3″,5; and one gave 4″; a mean of them is 3″,31; and the declination of the planet being 21° 57′ north, we have the apparent distance 45″,99; but I do not trust much to measures by time, in the manner these were taken without a system of wires in the focus of the eye glass, and with the clock and assistant at a considerable distance.

‡ The first satellite was 46° nf; the second was 2½° sf, and its distance from the planet was 381; this would give the greatest elongation 57″,36 which is probably much too large.

1787, March 18, 8ʰ 3′. The satellites are in the place where I expected them. The first is 5 or 6° np; the second is about 76° nf; it seems to be farther from the planet than when it was near the parallel. I attempted to measure its distance by the parallel wire micrometer; eclipsing the satellite with one wire, and bisecting the planet with the other. The measure gave the distance 46″,46.

‡ The first was 21° np; the second was 83½° nf; and its distance was 588; which gives the greatest elongation 47″,41.

1787, March 19. Both the satellites are in their expected situation, which for the first is 36° sp; for the second 79° np. At 7ʰ 48′ I took a good measure of the distance of the second satellite; it gave 44″,24. I attempted a second measure, but was interrupted before I had quite finished it to my liking; it gave 45″,98.

‡ The first satellite was 29½° sp; the second was 74⅘° np. and its distance 596. The expected situations, though calculated from imperfect tables, were sufficient to show that the satellites were not mistaken.

1787, March 20, 7ʰ 44′. I took three measures of the

distance of the second satellite; the first gave 40″,23: the second, with the remark, pretty full measure, gave 41″,89; the third with the addition, not too large, gave 40″,20.

‡ The satellite wa $52\frac{2}{3}°$ np, and the distance 564.

1787, April 9, 10^h 22′. I took two very accurate measures of the distance of the second satellite from the planet; the first gave 44″,54, the second 44″,35. By temporary tables its expected place was 57° sf.

‡ The satellite was 54° sf; and its distance 563.

1787, April 11, 9^h 18′. By temporary tables the expected situation of the second satellite was 4° sf. I took three good measures of its distance from the planet; the first gave 34″,47; the second 35″,32; the third 35″,74. A mean of them is 34″,99.

‡ The satellite was $1\frac{1}{2}°$ nf; and its distance 477.

1787, April 17, 8^h 53′. The two satellites are on opposite sides of the planet.

‡ The first was $40\frac{1}{2}°$ sf; distance 531. 9^h 6′, the second was $21\frac{2}{3}°$ np; distance 503.

1787, September 19, 15^h 55′. The first satellite (miscalled second) is 85° sp; the second (miscalled first) is about 30° sf.

‡ The first was $87\frac{1}{4}°$ sp; the second was $10\frac{1}{4}°$ sf. This being the first time of seeing the planet after its conjunction, accounts for the mistakes of the names.

1787, October 11, 16^h 49′. The first satellite (miscalled second) is 78° np. Two good measures of its distance from the planet were taken; the first gave 35″,18, the second 35″,96; a mean is 35″,57, 16^h 51′. The second satellite (miscalled first) is 40° sp.

‡ The first was 84° np, and its distance 599; the second was $52\frac{2}{3}$° sp; and its distance was 492. The long interruption in the observations was again the cause of a mistake of the names, which the calculation sets right.

1787, October 14, 15^h 59′. The angle of position of the first satellite by the micrometer is 48° 22′ sp; that of the second at 16^h 29′ is 66° 2′ sf.

‡ The first satellite was $49\frac{1}{3}$° sp; the second was $65\frac{1}{3}$° sf.

1787, October 20, 15^h 36′. Position of the first satellite by the micrometer 72° 0′ np. Position of the second at 16^h 8′, 80° 12′ np.

‡ The first was $76\frac{2}{3}$° np; the second was $80\frac{1}{2}$° np.

1787, November 9, 15^h 56. The second satellite is about 87° sf. The distance by four good measures 46″,15; 43″,92; 42″,94; 46″,57; mean 44″,89.

‡ The satellite was 84° sf; distance 594.

1788, January 14, 12^h 8′. The two satellites are almost in opposition; but the first precedes a line continued from the second through the planet.

‡ The first satellite was $77\frac{1}{4}$° nf; the second was 66° sp.

1789, February 22, 9^h 48′. The first satellite is about 80° sp; the second is about 85° sp; too much wind for measuring.

‡ The first satellite was $69\frac{1}{2}$° sp; the second was $85\frac{1}{3}$° sp.

1789, February 24, 9^h 13′. The first satellite is a few degrees more advanced in its orbit than the second.

‡ The first satellite was $48\frac{1}{3}$° sf; the second was 55° sf.

1789, March 13, 9^h 1′. The first satellite is 60° sf. 7^h 47′, the second is about 45° nf, third and fourth satellites were marked.

‡ The first satellite was $63\frac{1}{3}°$ sf; the second was $57\frac{2}{3}°$ nf. The third and fourth were found to be stars.

1789, March 14, 9^h 22′. The first satellite is 8° sf; the second is 70° nf.

‡ The first satellite was $19\frac{1}{4}°$ sf; the second was $81\frac{1}{4}°$ nf.

1789, March 16, 7^h 33′. The first satellite is 83° nf; the second is about 60° np. A third, about 2° sf; a fourth, about 8 or 10° np.

‡ The first satellite was $75\frac{1}{3}°$ nf; the second was 61° np. The third and fourth were stars.

1789, March 20, 7^h 50′. The two satellites were coarsely estimated to be at considerable angles sp.

‡ The first satellite was 63° sp; the second was $65\frac{1}{3}°$ sp.

1789, March 26, 10^h 44′. A star was mistaken for the first satellite; the second satellite (miscalled the first) is 45° nf.

‡ The first satellite was $63\frac{1}{4}°$ np; the second was 50° nf.

1789, December 15, 10^h 54′. The first satellite is about 71° sp. 10^h 49′, the second is about 75° sp; a third is about 75° sf.

‡ The first satellite was 72° sp; the second was $81\frac{1}{3}°$ sp; the third was a star.

1789, December 16, 10^h 12′. The first satellite is about 83 or 84° sf; the second is 85° sf. By the configuration they are very nearly in conjunction.

‡ The first was $83\frac{2}{3}°$ sf; the second was 83° sf.

1790, January 18, 9^h 32′. The first and second satellites are in the places I had calculated. There is a supposed third satellite about two diameters of the planet following, ex-

tremely faint and only seen by glimpses; 1^h 6′ after I could not perceive it; a fourth is about 70° np.

‡ The first was $38\frac{1}{4}$° sp; the second $85\frac{1}{4}$° nf.

1790, January 19, 9^h 34′. There is a very small star left in the place where the supposed fourth satellite was last night. 10^h 47′, I can see no fourth satellite near the second where it would be now if it had been a real satellite. With the assistance of a field bar to hide the planet; and a power of 300, I can see the first and second satellites very steadily, even the very first moment I look into the telescope.

‡ The first satellite was $76\frac{2}{3}$° sp; the second was $77\frac{1}{4}$° np. It is very strange that the third supposed satellite should not have been attended to when two observations are given to prove that the supposed fourth was not a satellite.

1790, January 20, 12^h 5′. The first and second satellites are in the places I had calculated; a third satellite is 45° np, and in a line with the planet and the second satellite.

‡ The first satellite was $77\frac{1}{4}$° sf; the second was $54\frac{1}{2}$° np. The third was not accounted for.

1790, February 6, 9^h 28′. I viewed the planet and satellites with three concave eye glasses, power about 240, 320, and 460. I see very clearly with these glasses. Cloudy.

‡ The first was $89\frac{3}{4}$° sf; the second was 64° sp.

1790, February 9, 9^h 19′. By a configuration the first satellite is at a considerable angle nf; the second at a great angle sf. A third is in a line with the planet and the second satellite; its distance from the planet by the configuration is about twice that of the second satellite.

‡ The first was $48\frac{3}{4}$° nf; the second was $61\frac{1}{2}$° sf; the third was $61\frac{1}{2}$° sf; two succeeding observations are decisive that

the supposed third satellite was not a star remaining in its place.

1790, February 11, 8° 30′. The satellites are in the places I had calculated. 8ʰ 56′, the small star of the 9th of February I believe is wanting; at least I cannot see it though the weather is very clear, but windy. An additional third and fourth are pointed out.

‡ The first satellite was 74° np; the second was 7° nf. The third and fourth of this night were found to be stars.

1790, February 12, 11ʰ 27′. The first and second satellites are in the places I had calculated. The third and fourth of last night are small fixed stars remaining in their places. The supposed third satellite of the ninth is not in the place where I saw it that night.

‡ The first satellite was 27° np; the second was 48½° nf.

1790, February 16, 8ʰ 2′. The first and second satellites are in the places I had calculated; the situation of a supposed third is described.

‡ The second was 56½° np; the supposed third proved to be a star.

1790, February 17. A configuration of stars situated in the planet's path is delineated.

1790, March 3, 7ʰ 58′. The first satellite is 40° sp. 8ʰ 42′, the second is 3 or 4° np.

‡ The first satellite was 56° sp; the second was 0½° np.

1790, March 5, 10ʰ 38′. The first and second satellites are in the places I had calculated; a 3d, with 600 is 56° sf; a fourth is delineated.

‡ The first satellite was 63° sf; the second was 66° sp; the third and fourth proved to be stars.

1790, March 8, 10^h 48′. Forty feet telescope. I saw the satellites with great ease. The speculum being extremely tarnished, I did not expect to have seen so well as I did.* Twenty feet telescope. The first satellite is 85° 7′ nf. 8^h 39′, the second is 67° 36′ sf. My wire is too fine and the power 460 too high for great accuracy.

‡ The first satellite was 79⅓° nf; the second was 59½° sf.

1790, April 8, 9^h 39′. The first satellite is on the opposite side of the second; the position of the second is 77° 53′ sf.

‡ The first satellite was 75° nf; the second was 76⅔° sf.

1791, January 31, 11^h 5′. The second satellite is 74 or 75° np. A supposed satellite in opposition to the second, and at double its distance from the planet, is marked in the configuration.

‡ The first satellite was 0¼° nf; its distance was 336, and not being noticed it was probably invisible; the second was 78¼° np; the supposed exterior satellite was 78¼ np.

1791, February 2, 8^h 23′. The first satellite is about 70° nf. 8^h 10′, the second is gone with the planet from the stars of the configuration of the 31st of January.

‡ The first satellite was 81° nf; the second was 30° np. The lettered stars of the configuration were all named as being left in their places, but the supposed exterior satellite of that day is not mentioned among them.

1791. February 4, 8^h 13′. The second satellite is 40° 48′ sp, but the measure is imperfect and may be out 5 or 6 degrees; a supposed satellite was marked, and a small star pointed out in the path of the planet.

* See Phil. Trans. for 1814. p. 275. A note relating to the polish of the 40 feet mirror.

‡ The second satellite was 52° sp. The supposed satellite was found to be a star.

1791, February 5, 11h 5′. The first satellite is 20° sp. 10h 45′, the second is 65° sp. With 600, third, fourth and fifth satellites are marked: but as they are also visible with 300, they are probably stars.

‡ The first satellite was 41° sp; the second was 74½° sp. The third, fourth and fifth, were lost for want of subsequent observations.

1791, February 22, 8h 25′. I cannot perceive the first satellite, probably owing to its nearness to the planet; I am pretty sure the orbits are contracted, so that the planet is approaching towards their node. 7h 30′, a measure of the position of the second satellite is 36° 18′ sf.

‡ The first satellite was 8¼° sp; distance 333, which may account for its not having been seen. The second was 39° sf.

1791, February 23, 7h 59′. Position of the first satellite 56° 33′ sp.

‡ The first was 60½° sp; the second was 7½° nf; the distance was 331, at which the satellite is sometimes invisible.

1791, March 1, 11h 47′. The two satellites are in the places I had calculated.

‡ The first was 73⅓° np; the second was 20¼° np.

1791, March 2, 9h 18′. The first satellite is hardly to be seen; it seems to be in about the most contracted part of its orbit; the second is only about two diameters of the planet from the edge of the disk, but the estimation cannot be very accurate, as I am obliged to hide the planet to see the satellite.

‡ The first was 37½° np; its distance 377. The second was 22½° sp; its distance 358.

1791, March 5, 7^h 56′. The first satellite is about 75° sp; the second about 85° sp; third, fourth, and fifth satellites were pointed out.

‡ The first satellite was 87° sf; the second was $89\frac{1}{3}$° sf; the third, fourth and fifth proved to be large stars, the nearness of the planet having diminished their lustre when observed as satellites.

1791, March 6, 12^h 2′. The first satellite is much nearer the planet than it was last night; the second is also nearer, but not much.

‡ The first satellite was $50\frac{1}{2}$° sf; the second was $70\frac{1}{2}$ sf.

1791, March 9, 9^h 52′. The first satellite, with a wire for the parallel and 300, is about 86° nf. The position being so near the perpendicular cannot be much out. By the micrometer it is 86° 25′ nf. 9^h 36′, the second is on the following side; it is nearer the planet than the first, and on that account appears smaller.

‡ The first satellite was $86\frac{1}{4}$° nf; distance 599; the second was $32\frac{1}{2}$° nf; distance 379.

1791, April 4, 8^h 48′. The first satellite is 84° 56′ nf; the second was not observed.

‡ The first was $82\frac{1}{4}$° nf; the second was $9\frac{1}{2}$° sf; its distance being 348 it might not be visible.

1791, December 19, 11^h 45′. I do not perceive the first satellite; the second is about 75° nf.

‡ The first was $7\frac{1}{4}$° sf, and its distance being 268, it was therefore invisible. The second was $82\frac{1}{2}$° nf.

1792, January 27, 11^h 58′. The first satellite was not observed; the second is about 40° nf, the estimation may be out 6 or 8 degrees. Cloudy.

‡ The first satellite was $20\frac{1}{4}°$ np; its distance being 283 it could not be seen. The second was $62\frac{1}{4}°$ nf.

1792, February 12, 8^h 28′. The first and second satellites are in the same line, and I measured their position together, it is 88° 19′ np; a supposed third is $84\frac{1}{2}°$ 23′ sp.

‡ The first satellite was $86\frac{1}{3}°$ np, and its distance 576; the second satellite was $83\frac{1}{4}°$ np; the third proved to be a star.

1792, February 13, 8^h 42′. Forty feet reflector, with 360, I saw the disk of the planet very well defined. Twenty feet. The satellites are advanced in their orbits; the first is drawn much nearer to the planet than it was yesterday; a very small star is 41° 22′ nf.

‡ The first satellite was $56\frac{1}{2}°$ np; its distance 389; the second was $66\frac{1}{3}°$ np. The very small star was left in its place.

1792, February 20, 12^h 57′. The first satellite is 89° 58′ nf; 13^h 8′, the second is 58° 21′ sf; a supposed third is 66° 17′ np.

‡ The first satellite was $83\frac{1}{2}°$ nf; the second was $53\frac{1}{3}°$ sf. By an increase of 25 or 30° in the angle of the third it was the same evening proved to be a star.

1792, February 21, 9^h 10′. Position of the first satellite 73° 52′ np.—9^h 30′, I suspected the second satellite to be in its calculated place, but even 600 would not verify it.

‡ The first satellite was $78\frac{2}{3}°$ np; the second was $17\frac{1}{3}°$ sf; distance 292.

1792, February 26, 11^h 30′. The position of the first satellite is 42° 49′ sf.—8^h 2′, that of the second is 73° 49′ np. A very small star between the planet and the second satellite is pointed out, and another towards the south at double the distance of the first is marked in the configuration.

‡ The first satellite was $52\frac{1}{4}°$ sf; the second was $75\frac{1}{4}°$ np. The small star was left in its place; but the distant one is not accounted for.

1792, February 28, 10^h 52′. The position of the first satellite is 69° 48′ nf. The second was not seen.

‡ The first satellite was $65\frac{1}{2}°$ nf; the second was $3\frac{1}{2}°$ sp; distance 293, and therefore invisible.

1792, March 15, 10^h 3′. I cannot see the first satellite with 300, 480, nor with 600. The second satellite is 73° 22′ sp.

‡ The first satellite was 17° sf; its distance was 302 and therefore invisible; the second was $74\frac{1}{2}$ sp.

1792, March 18, 8^h 19′. The first satellite is 82° 35′ np. 8^h 37′, the second is 60° 16′ sf.

‡ The first satellite was $81\frac{1}{4}°$ np; the second was 56° sf.

1792, March 19, 8^h 20′. The first satellite is 38° 4′ np, I see it very well notwithstanding it is near the planet. 8^h 42′, I cannot see the second with 300. With 480 I see it very well; I see it also with 800 and 1200; I tried 2400 and 4800, but a whitish haziness in the air prevents my seeing it with these powers.

‡ The first was 46° np and its distance $36\frac{4}{5}$; the second was 15° sf; its distance 299 which accounts for the difficulty of seeing it.

1792, March 23, 8^h 21′. I see the first satellite through flying clouds; the second is 89° 21′ np.

‡ The first satellite was $61\frac{2}{5}°$ sf; distance 480. The second was $88\frac{2}{5}°$ np.

1792, March 27, 11^h 6′. The first satellite (miscalled a very small star) is about 80° np; the second s by the configuration about 45° sp; a third (miscalled the first) was pointed out.

‡ The first satellite was 70° np; the second was 46½° sp; the third was not accounted for.

1792, March 30, 11h 18′. The first satellite by the configuration is at a great angle sp; the second is at a great angle sf.

‡ The first satellite was 78½° sp; the second was 83° sf. The first satellite (miscalled a star the 27th) is gone from the place where it was.

1793, February 5, 9° 18′. Neither the first nor second satellites are visible. A very small star is 19° 3′ sp.

‡ The first satellite was 30° sp; distance 282; the second was 23° sf; distance 245. There is no subsequent observation of the small star.

1793, February 7, 9h 38′. The first satellite is 79° 39′ sp. 9h 20′, the second is 59° 51′ nf. The wind being very troublesome, the measures cannot be very accurate: The difficulty was in finding the parallel.* I viewed the planet with 240, 320, 480, 600, 800 and 1200, but saw no satellites nearer than the two known ones. The north following satellite being farther from the planet than the south following one, I take it to be the second; the difference of their distance appeared plainest with 1200. I viewed the planet also with 2400, 3600, 7200.

‡ The first satellite was 86¾° sp. Distance 590. The second was 68¼° nf; distance 513; and supposing the radius of the orbit of the first to be to that of the second as 3 to 4, we have the apparent distance of the first to that of the second as 177 to 205.

* Telescopic vision in windy weather is generally very perfect, and except in cases which require an uninterrupted steadiness of the instrument, will admit of the highest magnifying powers.

1793, March 8, 11^h 21′. The first satellite is about 65° nf; the second is 90° nf; a third is about 75° nf.

‡ The first was $57\frac{1}{4}$° nf; the second was $89\frac{1}{3}$° nf; the third was a star.

1793, March 9, 10^h 35′. The first satellite is 85° nf; the second is about 82° np; a third is about 65° sp.

‡ The first was 80° nf; the second $77\frac{1}{2}$° np; there is no subsequent observation of the third.

1793, March 14, 9^h 37′. The first and second satellites are seen in their places; the situation of a third and fourth is pointed out. The first satellite is brighter than the second.

‡ The first was $89\frac{1}{3}$° sf, and 16° 26′ from its greatest elongation; distance 574. The second was 80° sp, and 7° 56′ from its greatest elongation; distance 588. The superior brightness of the first therefore could not arise from its greater distance. The third and fourth supposed satellites had no subsequent observations.

1793, April 3, 10^h 53′. The first satellite is 50° nf; the second is 80° nf.

‡ The first was $52\frac{2}{3}$° nf; the second was 79° nf.

1794, February 21, 8^h 24′. The first satellite is about 88° nf; the second is about 86° nf.

‡ The first was 89° nf; the second 85° nf.

1794, February 25, 8^h 24′. By a configuration the first satellite is at a great angle sp; and its distance from the planet is greater than that of the second, which is at a much smaller angle sp. Several small stars are pointed out.

‡ The first satellite was 84° sp; distance 593. The second was $47\frac{1}{4}$° sp; distance 323. The stars were left in their places.

February 26, 8^h 28′. The first satellite is 70° 53′ sf. 8^h 7′, the second is 66° 56′ sp; many small stars are pointed out.

‡ The first satellite was 78° sf; the second was $67\frac{1}{3}$° sp; the stars remained in their places.

1794, February 28, 9^h 43′. The first satellite is 62° 55′ nf. 9^h 26′, the second is 86° 44′ sp. 8^h 15′, there is a very small star which I did not see the 26th; it is brighter than a lettered star not far from it. Its position is pointed out by the stars of the configuration.

‡ The first satellite was $61\frac{1}{2}$ nf; the second was $87\frac{1}{2}$° sp. The stars remained in their places. The position of the small star of the 26th, by identification was about 24° nf.

1794, March 2, 8° 25′. The first satellite seems to be at its greatest elongation; the second satellite was not seen.

‡ The first was 86° np; distance 507; the second was $55\frac{1}{4}$° sf; distance 275, therefore not visible.

1794, March 4, 11^h 22′. I can see neither the first nor the second satellite. A third satellite is 61° 32′ nf. Many small stars are pointed out.

‡ The second satellite was $57\frac{1}{2}$° nf; its distance 383; it was therefore visible, and its position agrees with the measure taken of a satellite miscalled the third: the inaccuracy of my tables in 1794, occasioned the mistake. The small stars remained.

1794, March 5, 11^h 10′. The first satellite is 75° 50′ sp. 10^h 57′, the second is 72° 27′ np. There is no star in the place where the supposed third was last night. Many small stars are again pointed out.

‡ The first was 76° sp; the second was $72\frac{1}{4}$° nf. The absence of the miscalled third confirms the mistake, and

is a proof of the great attention that was paid to ascertain the nature of supposed satellites. The small stars remained.

1794, March 7, 11^h 18′. I cannot perceive the first satellite. 10^h 57′, the second is nearer the planet than it was the last time I saw it. Small stars are pointed out.

‡ The first was 63° sf; distance 320, invisible. The second was $87\frac{1}{4}$° np. There is no subsequent observation of the small stars.

1794, March 17, 7^h 38′. I can see neither of the two satellites.

‡ The first was 41° nf, distance 292, invisible. The second was $98\frac{3}{5}$° nf; distance 285, invisible.

1794, March 21, 11^h 53′. I cannot see the first satellite. I looked at several different hours for it. 10^h 53′, the second satellite is 88° 8′ np. 9^h 19′, the place of a small suspected star is pointed out, but it cannot be verified with 460 and 600.

‡ The first satellite was 27° sp; distance 248, invisible. The second was $81\frac{1}{3}$° np. The suspected star was seen in its place the following night.

1794, March 22, 8^h 47′. There is no mention of the first satellite; the second is 61° 46′ np.

‡ The first was $66\frac{1}{4}$° sp, and its distance being 480, it must have been seen and taken for a star. The second satellite was $60\frac{1}{4}$° np; distance 311.

1794, March 23, 8^h 32′. The first satellite is one of two small stars that are south of the planet; it is the preceding and largest of the the two. 8^h 42′, the second satellite is not visible.

‡ The first was 82° sp, 1° 26′ past its greatest elongation The second was 1° np, 1° 49′ past its shortest elongation, distance 207; invisible.

1794, March 26, 9ʰ 2′. Position of the first satellite 61° 53′ nf, as accurate as the faintness of the satellite will permit. 8ʰ 48′, the second satellite is 77° 0′ sp. very accurate. 9ʰ 17′, I suspect a third satellite directly north a little farther from the planet than the first, and the power 480 almost verifies the suspicion. 9ʰ 26′, with 600 I still suspect the same, but cannot satisfy myself of the reality. 11ʰ 24′, I see the supposed third satellite perfectly well now; it is much smaller than the first, and in a line with the planet and the first. An extensive configuration is delineated.

‡ The first satellite was 56⅔° nf; the second was 79½° sp; the third satellite being in the position of the first at 11ʰ 14′, must have been 59½° nf.

1794, March 27, 10ʰ 25′. The first satellite is 75° 59′ nf. 10ʰ 12′, the second is 88° 35′ sf. 8ʰ 15′, the small star observed last night at 11ʰ 14′ is gone from the place where I saw it. From its light last night compared to a star marked *r* in the configuration which to night is very near the planet, and scarcely visible, I am certain that it must be bright enough to be perceived immediately, if it were in the place pointed out by the configuration. 11ʰ 19′, I have many glimpses of small stars, one of them is in a place a little north following the first satellite, agreeing with what would probably be the situation of the third satellite of last night if it had moved with the planet. A supposed third of this evening is preceding the first satellite, but nearer the planet. A supposed fourth is sf; its distance is almost double that of

the second satellite. Some other small stars or supposed satellites are seen to the south at a good distance.

‡ The first satellite was 79° nf; the second was 81¼° sf. The fourth proved to be a star.

1794, March 28, 9ʰ 12′. The first satellite is 88° 31′ np. 9ʰ 1′, the second is 82° 7′ sf.

‡ The first satellite was 87° np; the second was 78° sf.

1794, April 1, 9ʰ 14′. The first satellite i 83° 2′ sp. 9ʰ 23′, the second is 70° 26′ nf.

‡ The first satellite was 87¼° sp; the second was 72° nf.

1796, March 4, 11ʰ 10′. The Georgian planet is about 13′ of space preceding and 5 or 6′ north of a nebula. An extensive configuration was made, but no satellites were noticed.

‡ The nebula was No. 272 of the first class of my third catalogue. (See Phil. Trans. for 1802, page 505.) The first satellite was 42¼° sp; distance 158, invisible. The second was 87½° np, distance 364, and after an interval of near two years might possibly be overlooked. The stars pointed out remained in their places.

1796, March 5, 12ʰ 25′. The first satellite is 72° 20′ sp. 10ʰ 53′, I suspect a very small star between two of the lettered stars of last night's configuration which at the time it was made was not there. I had a pretty certain glimpse of it.

‡ The first satellite was 75° sp.—10ʰ 25′, the second was 72° np; distance 123, invisible. By the configuration the suspected star was at a considerable distance, about 72° np.

1796, March 9, 11ʰ 35′. The first satellite is 70° 36′ nf. 11ʰ 22′, the second is 83° 35′ sp. As the probability is that

other supposed satellites move in the same plane with the first and second, I chiefly look for them in the direction of the position of their orbits which is now nearly a straight line; a star that may possibly be a distant satellite is pointed out.

‡ The first satellite was $70\frac{1}{3}°$ nf. The second was 81° sp; the star remained in its place.

1796, March 10, 11^h 48′. The first and second satellites by a configuration, are not far from being in opposition, the first not being come to a line drawn from the second through the planet. With 600 there is no star between the satellites and the planet that may be supposed to be an inner satellite; with this power the satellites are very large and visible, I see them better than with a lower.

‡ The first satellite was $79\frac{1}{4}°$ nf; the second was $86\frac{1}{3}°$ sp.

1796, March 27, 10^h 6′. The first satellite is in the place I had calculated.

‡ It was $75\frac{1}{2}°$ nf.

1796, March 28, 10^h 7′. The first and second satellites are in the places I had calculated; the apparent contraction of their orbits is such as to approach to a straight line.

‡ The first satellite was $84°$ nf; the second was $74°$ nf.

1796, April 4, 11^h 16′. The first satellite is not visible; the second is near a small sp. star. There is no star in the transverse of the apparent elliptical orbit that could be taken for a satellite, unless that near the second should be one going towards its greatest elongation, or coming from it.

‡ The first satellite was 68° nf; its distance 415; how it happened not to be visible I cannot account for; the configuration has no star near the place; the second satellite was $76\frac{1}{3}°$ sp. The star near the second remained in it former situation.

1796, April 5, $10^h\ 48'$. The first and second satellites are apparently in opposition, the same wire covers them both and the planet. There is no star in the line of the transverse that can be taken for a satellite: the night being beautiful, I examined that line with 300 at a distance, and with 600 within the orbit of the two satellites.

‡ The first satellite was $74\frac{2}{3}°$ nf; the second was $81\frac{2}{3}°$ sp.

1797, March 15, $9^h\ 44'$. The first and second satellites are not far from their opposition; by the configuration they are short of it.

‡ The first was $78\frac{1}{4}°$ sp; the second was $81\frac{1}{4}°$ nf.

1797, March 17, $9^h\ 51'$. The first and second satellites are both invisible; the night is very beautiful and I have a field bar to hide the planet; but notwithstanding this, I cannot see either of the satellites; many stars are pointed out.

‡ The first was $69\frac{1}{4}°$ sf; distance 110, invisible. The second was 76° np; distance 126, invisible. The stars had no subsequent observation.

1797, March 21, $10^h\ 9'$. The first satellite is not visible; the second is nearly at its greatest elongation, it is about 70° south preceding; many stars are pointed out.

‡ The first satellite was $88\frac{1}{2}°$ np; distance 240, invisible; the second was $79\frac{1}{2}°$ sp; distance 588. The stars remained.

1797, March 23, $10^h\ 28'$. With 320, I see neither of the satellites. $10^h\ 32'$, having just been told where the second should be, I perceived it in its place; with 600 I see it very well; many stars are pointed out.

‡ The first satellite was 76° sp; distance 518; it does not appear why it could not be seen. The second was 88° sp; distance 298. The stars remained.

1797, March 25, $10^h\ 40'$. With 320 I cannot perceive either of the satellites; with 600 I can see neither of them; many stars are pointed out.

‡ The first satellite was 87° sp; distance 320, invisible. The second was $66\frac{2}{3}$° nf; distance 251, invisible. The stars remained.

1797, March 28, $10^h\ 36'$. The two satellites seem to be nearly in a line drawn from the second to the centre of the planet; the second is 80° 26′ nf. There is an exceeding small star about four times the distance of the second satellite in the line of the greatest elongation, I do not remember to have seen it among the lettered stars which are pointed out, the 25th.

‡ The first satellite was $78\frac{1}{2}$° nf; distance 597. The second was $80\frac{1}{3}$° nf; distance 592. There is no subsequent observation of the small star.

1797, March 29, $9^h\ 59'$. I see one of the satellites. Cloudy. I suppose it to have been the second.

‡ The first was 83° nf; the second was $83\frac{1}{3}$° nf.

1798, February 6, $11^h\ 29'$. The second satellite (miscalled a star) is at a great angle sp; three stars are pointed out.

‡ The first satellite was $74\frac{1}{3}$° nf; distance 251; invisible; The second was 79° sp; distance 552. The three stars remained.*

1798, February 7, $11^h\ 7'$. I cannot see the first satellite. The second is at a great angle sp near one of the stars marked down last night, which is now so small that I cannot distin-

* The planet being past the node, the angular distance of the satellites from zero, and their apparent motions are inverted.

guish it from the satellite. An extremely small star preceding a line that joins two lettered stars may be an exterior satellite. Position of the satellite (called the 6th) 64° 41′ np. 12^h 36′, there is no star in the path of the planet which to morrow or next day can be taken for this 6th satellite.

‡ The first satellite was 85° sp; distance 159, invisible. The second 78¼° sp; distance 592. The supposed satellite, called the 6th by way of readily referring to it, and also partly to express its distance, was found to remain in its observed place.

1798, February 11, 9^h 17′. The path of the planet is marked by a configuration of lettered stars, taking in those from which it comes and those towards which it will go.

11^h 35′, the situation of a supposed exterior satellite (called the 5th) with regard to the lettered stars is pointed out. It is excessively faint, but the night is very beautiful.

11^h 46′, the position of the 5th satellite is 89° 19,5 nf; but the satellite is so faint that the measure cannot be very accurate.

12^h 16′, I cannot see either of the two old satellites. There is an extremely small north preceding star *x*, which may be a more distant satellite; it is much smaller than the 5th, and will therefore become invisible when the planet comes near it.

‡ The first satellite was 89¼° nf; its distance was 41, and it was therefore invisible. The second was 79½° nf; its distance was 241; and it was therefore also invisible.

1798, February 13, 10^h 17′. The old satellites are in the place I had calculated. The 5th satellite and the small star *x*, observed February 11, are not visible; but the weather is very indifferent.

11^h 49′, I do not see the 5th satellite where it was February 11.

12^h 0′, the position of the two old satellites is 76° 48′ nf.

12^h 15′, I see the extremely small star x remaining in its former place.

12^h 44′, the first and second satellites are exactly in a line pointing to the centre of the planet. A second measure of their position is 75° 45′. I cannot see the 5th satellite in the place where it was February 11.

‡ The first satellite was 78¼ nf; distance 594. The second was 78¼° nf; distance 576.

1798, February 15, 11^h 21′. There is a very small star in the line of the greatest northern elongation, it may possibly be an interior satellite, the first and second being invisible.

I see the extremely small star x of the 11th perfectly well, but the 5th satellite of the same night is gone from the place where it was that evening. It was considerably brighter than x, so that if it were in its place, I must certainly see it.

11^h 41′, the star which at 11^h 21′ I supposed might be an interior satellite is too far from the planet; it may possibly be the 5th satellite of the 11th on its return from the northern elongation towards the planet.

I believe there is another satellite or star between this last mentioned one and the planet; I do not suppose the second satellite to be visible, otherwise it would agree well enough with the situation of the star between the 5th and the planet. By the configuration the intermediate star is at about half the distance of the farthest of the two.

12^h 13′, position of the supposed 5th satellite 84° 49′ nf.

‡ The first satellite was 73° nf; distance 124, it was there-

fore invisible and could not be the supposed 5th satellite. The second was 77° nf; distance 455; it was therefore visible, and agrees very well with the satellite miscalled the 5th; the star between the second satellite and the planet must have been an interior satellite at its greatest northern elongation. At the time of observation, my defective tables made me suppose the nearest of the two to be the second satellite.

1798, February 16, 9^h 25′. The supposed fifth satellite observed last night at 11^h 41′, I believe is gone from the place where I saw it at that time. The night is very beautiful; the planet however is still low, and I shall look for it again when it is higher.

10^h 57′, the supposed fifth satellite is gone from its former place. It was so visible last night when near the planet, that I should certainly see it without difficulty if it remained in the same place, as the planet is now removed from it.

11^h 5′, the first and second satellites are invisible.

11^h 19′, there is a very faint satellite in the southern elongation; probably the sixth; and if it be the sixth satellite it is probably a little before or after its greatest elongation. It is excessively faint.

12^h 27′, the weather is not so clear now, though still fine, but the sixth satellite cannot be seen; it is plain, therefore, that the least haziness will render it invisible.

‡ The first satellite was 80½° sp; distance 289, invisible; the second was 75½° nf; distance 244, invisible. The interior satellite observed the fifteenth, being taken for the second, was not looked after; but as the supposed fifth was scrupulously ascertained to be removed, the interior satellite, had it been a star remaining in its former situation, must unavoidably

have been seen; for by the configuration they could not be much more than a diameter of the planet asunder.

1798, February 18, 9^h 19′. I see the sixth satellite observed February 16, at 11^h 12′, it has left the place where it was at that time. It is nearer the planet than it was that evening, I suppose it therefore to be on its return from its southern elongation.

There is a seventh satellite near the sixth, rather a little fainter than the sixth; a supposed eighth satellite is pointed out.

11^h 25′, the position of the sixth satellite is 80° 55′ sp.

11^h 31′, with 480 I see the satellite near the sixth perfectly well; the distance between the two is about $\frac{1}{4}$ or one diameter of the planet.

11^h 44′, I see the satellite much better with 600; that which is farthest from the planet is the largest.

‡ The first satellite was 78° sp; distance 563. This therefore might be the satellite which was seen near the sixth. The second satellite was $80\frac{1}{2}$ sp; distance 290, and was invisible. The supposed eighth, proved to be a star.

1798, February 19, 11^h 12′. The first satellite is invisible; the second is near the greatest elongation.

‡ The first was 81° sp; distance 269; invisible. The second was $79\frac{1}{2}$° sp; distance 490.

1798, February 22, 9^h 9′. The first satellite by its distance is not far from its greatest northern elongation; it is very large. There is a satellite to the south exactly opposite to the first; it is very small but may be the second. The moon is too bright to see very faint satellites.

‡ The first satellite was 78° nf; distance 594. The second

was 78° sp; distance 481. The small appearance of the second satellite is not easily to be accounted for; its distance from the planet was not much less than that of the first; for if the greatest elongations of the satellites be as 3 to 4, the above distance will be 1782 to 1784.

1798, February 26, 9^h 52′. The first and second satellites are in opposition; the first being sp, the second nf. The moon is so bright that their light is very feeble. Position 79° 53′ from sp to nf. The first satellite is small, the second is large.

‡ The first satellite was 78½° sp; distance 584. The second was 79° nf; distance 527. The different proportional light of the satellites in different situations, will lead us to suppose that they have a rotation on their axes. The twenty-second, when the second satellite was 78° sp, it was fainter than the first, and this evening when it was nf, it was brighter.

1798, March 11, 8^h 13′. With 300 the first and second satellites are close together, like a very faint double star of the first class. The second satellite is the most north of the two.

9^h 45, the position of the two satellites is 78° 15′,3 nf. There is hardly a division between them.

‡ The first satellite was 78¼° nf; distance 580. The second was 78¼° nf; distance 445; and supposing the diameter of the orbits of the two satellites to be as 3 to 4, their distances at the time of observation would be as 174 to 178.*

1798, March 12, 9^h 11′. The first satellite is nearly at the same distance from the planet as it was last night; the second

* The angular distance of the satellites from zero and their apparent motions are reverted.

is farther from the planet. The two satellites and the centre of the planet are exactly in a line. Their position is 78° 12′,6. With 480 I had a glimpse of a south preceding satellite; but could not verify it with 600.

12^h 6′. Distance of the second satellite 50″,02. I contrived to throw a little light upon the wires, as the satellite was bright enough this evening to bear it.

‡ The first satellite was 78¼° nf; distance 528. The second was 78¼° nf; distance 577.

1798, March 13, 11° 46′. The first satellite is invisible. The second is much nearer the planet than it was last night. The weather is not clear, owing to easterly winds.

‡ The first satellite was 78¼° nf; distance 170; invisible. The second was 78¼° nf; distance 577.

1798, March 14, 11^h 55′. The first satellite is invisible; 8^h 31′, the second is still at a considerable distance south preceding.

11^h 47′, twenty-five reflector, power 200. The Georgian planet is better defined in this instrument than I have ever seen it before. With 300, its disk is as sharp and well defined as that of Jupiter. The second satellite is brought to a sharp point. A little while ago I had a glimpse of a south preceding satellite, and just now I have seen it again. 12^h 0′, I cannot verify the satellite, but can hardly believe it a deception.

Twenty feet reflector, power 300. I tried to measure the distance of the second satellite, but its present faintness will not afford light enough to see the wires of the micrometer.

‡ The first satellite was 78¼° sp; distance 260, invisible; but the 25 feet telescope with a mirror of 24 inches in

diameter, it appears, had light enough to show it. The second satellite was $78\frac{1}{4}°$ nf; distance 473.

1798, March 16, 8^h 37′. I see a south preceding satellite at a good distance; it may be the first at its greatest elongation, but it is certainly smaller than it should be; unless the state of the air should be worse for seeing than it appears to be.

9^h 31′, the distance of the first satellite is 36″,05. The satellite is so faint that it is impossible to be very accurate; it will not bear any light to the wires.

11^h 34′, twenty-five feet reflector. With 300 I see the satellite very distinctly, but the evening is not fine.

‡ The first satellite was $78\frac{1}{4}°$ sp; distance 576. The second at 8^h 28′ was $78\frac{1}{4}°$ sp; distance 13; invisible.

1798, March 18, 8^h 26′. The first satellite is invisible; the second is south preceding at a considerable distance; it is farther off than the greatest elongation of the first.

‡ The first satellite was $78\frac{1}{4}°$ nf; distance 51; invisible. The second was $78\frac{1}{4}°$ sp; distance 484.

1798, March 19, 9^h 51′. I see a north following satellite which I suppose to be the first. The second is near its south preceding greatest elongation. Distance 49″,90, I can only apply a very distant lantern, which will hardly give light enough to show the wires. The satellite is not so bright in its southern elongation as it was March 12th in its northern one, though the weather is now very beautiful. In the south preceding elongation is a distant star that may be a satellite; many other stars are pointed out.

‡ The first satellite was $78\frac{1}{4}°$ nf; distance 445. The

second was 78¼° sp; distance 588. The stars remained in their places.

1798, March 21, 10ʰ 25′. The first satellite is north following; it is faint, and at nearly the same distance from the planet as it was in March 19. The second satellite is near one of the stars pointed out the 19th, both being at the same distance from the planet. 10ʰ 40′, there is such a multitude of small stars in the neighbourhood of the planet, that it would be endless to look for the additional satellites among them.

‡ The first satellite was 78¼° nf; distance 488. The second was 78¼° sp; distance 409.

1798, March 22, 10ʰ 35′. The first and second satellites are both invisible.

‡ The first was 78¼° nf; distance 58; invisible. The second was 78¼° sp; distance 173; invisible.

1798, April 6, 8ʰ 31′. The first satellite is north following; I suspect the second to be between the first and the planet, but cannot verify the suspicion. There is a supposed south preceding satellite, but it is too near the planet to be seen steadily.

‡ The first satellite was 78° nf; distance 564. The second was 75⅓° nf: distance 230; it was therefore the satellite suspected between the first and the planet. The supposed south preceding satellite was lost among the numerous small stars.

1798, April 7, 9ʰ 26′. There are two satellites north following; they are very near together. The distance between them is less than half the diameter of the planet. The centre of the planet and the two satellites are exactly in a line.

There are so many small stars that it is next to impossible to look for the additional satellites.

‡ The first satellite was 79° nf; distance 546. The second at 8^h 45′ was $77\frac{1}{3}$° nf; distance 453.

1798, April 8, 10^h 19′. There is no satellite visible between the second and the planet; the second satellite is north following, at a greater distance from the planet than last night. There is a very small star at a little more than twice the distance of the second satellite north following.

‡ The first satellite was $80\frac{1}{2}$° nf; distance 289; invisible. The second was 78° nf; distance 576. The small star remained in its place.

1798, April 9, 9^h 34′. I cannot see the first satellite. The second is at a distance north following, rather farther from the planet than last night. With 480 and 600, there is no satellite between the second and the planet.

‡ The first was $74\frac{1}{2}$° sp; distance 171; invisible. The second was 79° nf; distance 578.

1798, April 11, 9^h 8′. The first satellite is south preceding at a considerable distance; but not at its greatest elongation. I cannot see the second satellite. I suspect a very small star in the line of the north following greatest elongation, a little farther from the planet than the first satellite. With 480 I cannot verify the suspicion.

‡ The first satellite was $78\frac{1}{2}$° sp; distance 587. The second was $80\frac{1}{4}$° nf; distance 247; invisible. This satellite could hardly be the suspected star, as it was but at little more than half the distance of the first satellite from the planet.

1798, April 12, 9^h 54′. I cannot see the first satellite, nor the second. With 480 there is no satellite either new or old

visible. The night seems to be very clear, but the wind is in the north-east.

‡ The first satellite was $80\frac{2}{3}°$ sp; its distance was 382, and it ought to have been seen. The second was 61° sp; distance 46; invisible; if the north-east wind, which is always unfavourable for astronomical observations, prevented my seeing the first satellite, it could not be expected that the suspected star of the eleventh would be visible.

1798, April 13, 9^h 5′. The first satellite was not seen. There is a south preceding satellite at a little greater distance than half the greatest elongation of the first. It took some time to verify its existence. 400 shows it very well; it is the second satellite. The very small stars in this neighbourhood are so numerous, that it is impossible to look for the new satellites among them. A great many of the stars are pointed out.

‡ The first was 1° sf; distance 16; invisible. The distance being so small, the method here used is not sufficient to give an accurate position. The second satellite was 76° sp; distance 301.

1798, May 3, 10^h 3′. The first satellite is north following. 10^h 7′, I suspect another north following a little nearer the planet than the first; 460 almost verifies it.

‡ The first satellite was $79\frac{1}{3}°$ nf; distance 556. The second, which was the suspected one, was $74\frac{1}{2}°$ nf; distance 259; it was therefore seen at a distance considerably less than half the greatest elongation.

1799, April 3, 9^h 51′. I viewed the Georgian planet with 157 and 300; one of the satellites is about 10° sp.

‡ The first satellite was 77° sp; distance 581; the second

was 66° sp; distance 172; this being invisible and the first at so great an angle, a star must have been taken for a satellite, which might well happen after an interval of eleven months. The observation was chiefly made to try two newly polished mirrors.*

1799, April 8, 9^h 51′. Both the old satellites are in a line near the greatest north following elongation. The nearest is very faint.

‡ The first satellite was $74\frac{1}{3}°$ nf; distance 452; it was the faint satellite and the nearest of the two. The second was $75\frac{1}{4}°$ nf; distance 529.

1800, March 26, 11^h 0′. With a new mirror and power 900 I saw the planet beautifully well defined, and one of its satellites south preceding.

‡ The first was $74\frac{1}{4}°$ sp; distance 566, it was therefore the observed satellite. The second was $31\frac{2}{3}°$ nf; distance 134; invisible.

1800, April 26, 9^h 30′. I see two satellites almost in opposition; the south preceding one is the largest and at the greatest distance. I see also several extremely small stars; but without a regular succession of observations, it is impossible to determine whether any of them may be satellites.

‡ The first satellite was 71° nf; distance 469. The second was $76\frac{1}{3}°$ sp; distance 592. They were the observed satellites, and the apparently inverted direction of their motion is evident.

1801, March 8, 12^h 0′. The first satellite is at a great angle south preceding; the second at a great angle north

* The planet being now again past the node, the angular distance of the satellites from zero, and their apparent motions are again inverted, and will remain so.

following; but a line from the second drawn through the planet, leaves the first satellite on the following side.

‡ The first was $85\frac{1}{4}°$ sp; distance 588. The second was $78\frac{1}{4}°$ nf; distance 599.

1801, April 17, 10^h 30′. The first and second satellites are in view at great angles north following the planet. There is a third satellite at a great angle south preceding; in the configuration it is marked exactly in opposition to the second, and at half the distance of the first. Six stars are pointed out.

‡ The first satellite was $77\frac{1}{4}°$ nf; distance 598. The second was 81° nf; distance 586; and the third by the configuration was 81° sp.

1801, April 18, 10^h 26′. The first and second satellites are in the configuration at great angles north following. The six stars of last night are in their places, but I do not see any star where the third satellite was marked.

‡ The first satellite was 65° nf; distance 488. The second was 74° nf; distance 578. The third was probably the interior satellite at its greatest southern elongation, which cannot be visible two days together. The configuration of this evening, compared with that of the night before, shows by the situation of the satellites, that their apparent motion is in an inverted direction.

1801, April 19, 10^h 24′. The first satellite was not observed. I saw the second satellite advanced in its orbit in the inverted order. The moon is too bright to make observations on additional satellites.

‡ The first was $7\frac{1}{2}°$ nf; distance 144; invisible. The second was 66° nf; distance 438.

1808, May 27, 10^h 0′. The planet is too low to admit the

use of very high magnifying powers. With 300, however, I have a glimpse of what I suppose to be the two large satellites. A haziness coming on will not permit the angle of position to be taken.

‡ The first was 47° sf: distance 478; the second was 56° np; distance 509. They were therefore both visible.

1809, May 12, 11^h 0′. I viewed the planet with 300, but could not perceive the satellites. The planet is too low, and there is a strong twilight.

‡ The first satellite was $72\frac{1}{2}$° sp; distance 578. The second was 53° np; distance 529.

1810, May 25, 10^h 40′. I viewed the Georgian planet with the 40 feet telescope, power 400. The disk of it is very bright. Several small stars are near it, but without a series of observations, it cannot be possible to ascertain which of them are satellites. What I suppose to be the second is 65° or 70° nf.

‡ The first was $87\frac{1}{4}$° np; distance 594. The second was 76° np; distance 588. Both satellites were therefore visible, but being among surrounding stars, could not be distinguished from them.

Investigation of several particulars deduced from the foregoing observations, with an exposition of the method by which they have been obtained.

The first use to be made of the numerous angles of position that have been taken, must be an investigation of the place of the node, and the inclination of the orbits of the satellites. When these two particulars are obtained, the times of the periodical

revolutions of the satellites about the planet, may be settled. It will then be necessary to calculate the places of the satellites for the times in which they were observed, in order to identify them; for, notwithstanding all possible care was taken to keep them in view, yet after the long unavoidable annual interruptions, and the periodical interference of the moon, it will be seen, that several mistakes have been made in naming the satellites, which by that means may be easily corrected.

The place of the ascending node, the inclination of the orbits, and the retrograde motions of the satellites determined.

When the observations of the satellites in the year 1797, and the beginning of 1798, are examined, it will be found that the first satellite could seldom be seen, and that its positions, when observed, were always at a great angle from the parallel; the second was also frequently invisible; and its observed positions were likewise at great angles. From these appearances it may be concluded that the planet was approaching to the node of the satellite's orbits. At the latter end of February, and the beginning of March 1798, the position of both the satellites approached to a settled angle, which at last, for two successive days, namely the 11th and 12th of March, remained stationary; I have therefore supposed that the planet was then in its passage through the node, and have in my calculations admitted its place to be five signs, 15 degrees, 30 minutes.

Then a mean of the angles of position 78° 15′,3 and 78° 12′,9 taken the 11th and 12th of March, being 78° 14′ north following, it will appear from the method of calculation

which will be explained, that the inclination of the orbits of the satellites to the ecliptic is 78° 58′.

From these data it also follows, that the motion of the satellites in their revolutions round the planet, by which they are carried from their ascending node to their greatest elongation, is retrograde.

Consideration of the principles by which the periodical revolution of the satellites may be obtained from the observed angles of position.

It will be necessary to premise, that, in order to simplify the investigation of the periodical revolution of the satellites, I have supposed the orbit of the Georgian planet, which differs only about $\frac{1}{4}$ of a degree from the ecliptic, to be coincident with it.

When the rate of the motion of a satellite in its orbit is to be determined from two observed situations, it is required to reduce its first apparent place on the plane of projection, to its real situation in its orbit, I have therefore taken the ascending node for a fixed point, from which we may begin to number the degrees of the satellite's situation; then, as in the second observation, the satellite must also be brought from its apparent place to its real one, we have to allow for three material alterations, that will more or less affect the calculation, according to the length of the interval of time between the two observations.

The first of these alterations is that which takes place in the situation of the parallel, from which the angles are measured; the second is a change in the inclination of the plane of the orbit of the satellite to the plane of projection; and the

third is an alteration in the distance between the ascending node and the extreme point of the transverse axis of the ellipsis, into which the orbit is projected.

In figure 1, (Pl. XVI.) suppose the circle PSFN to represent the plane of the orthographic projection, in which the angles of position are counted from the parallel P and F towards S and N, and expressed by the number of degrees 10, 20, 30 to 90 in each quadrant. Then let there be a moveable circle within the former, of which the degrees should be marked in succession, 10, 20, 30 continued to 360. The inner circle being moveable, the line from 180 to 360 will express, by its different situations, the position of the transverse axis of the ellipsis into which the orbits of the satellites at any given time are projected. The conjugate extending from 90 to 270, and the lines parallel to it, will point out the direction in which the orbits will be more or less contracted according to the different inclinations of their planes to the plane of the projection.

In a triangle PSp, figure 2, let P be the pole of the ecliptic. S, a point in the orbit of the satellite, 90 degrees distant from the ascending node. p, the point of the greatest northern elongation of the satellites, which on the moveable circle is marked 360. It is the zero of the ellipsis into which the orbits of the satellites are projected, and its calculated situation will regulate the adjustment of the moveable circle. N, a point in a meridional direction, 90 degrees distant from the geocentric place of the planet. The arch PS then being the complement of the inclination of the satellite's orbit to the ecliptic, is therefore given; and the angle at P is equal to the distance of the longitude of the planet from the node. The

angle at S is a right one; and the arch PN, being the measure of the angle of position of that point of the ecliptic where the planet is situated, may be had by Table LIII, published in Dr. MASKELYNE's first volume of Observations.

To find what alteration has taken place in the situation of the parallel with regard to the point *p*, we calculate the position of this point for any given time by the following analogy. (1) cos P : rad : : tan PS : tan Pp; and the difference between PN and Pp will give Np; the complement of which is the position of *p*, with regard to the parallel. When P*p* is greater than PN, the position of P being north following, that of *p* will be north preceding; but when P*p* is less than PN, it will be north following.

To find the inclination of the plane of the satellite's orbit to the plane of projection, we have only to calculate the distance of the poles of these planes; and since the place of the planet is the pole of the projecting plane, and since also the situation of the pole of the orbits is known, which is in longitude 15 degrees 30 minutes of Gemini, and latitude south 11° 2′, therefore in the right angled triangle figure 3, of which GL is the distance of the planet from the longitude of the pole, and PL the given latitude of the pole, we have the analogy (2) rad : cos GL : : cos PL : cos GP; which is the required inclination of the two planes to each other.

To find the distance of the point *p*, or zero of the projected ellipsis from the ascending node, by figure 2 we have the analogy (3) rad : sin PS : : tan P : tan S*p*; and 90° + S*p*, before the node, and 90°—S*p*, after it, will give the distance of *p*, or zero point of the inner circle from the ascending node.

The periodical revolutions of the satellites determined.

In the natural order of investigating the motions of the satellites, the first consideration ought to be to identify the observations, lest a star should have been mistaken for a satellite, or one satellite for another; but as the calculations required for this purpose cannot be made without proper tables of their periodical revolutions, I have proceeded in the following manner.

The earliest angles of the positions of the satellites which appeared to be sufficiently accurate for the purpose of settling their motions were taken 1787, February 16, $9^h\ 38'$ mean time. With a wire for the parallel in the focus of the eye-glass, and a magnifying power of 300, the position of the first satellite was five degrees north following; and that of the second was three degrees south following: the motion of the satellites being so near the parallel, there can be no material error in the estimation of the angles; and to prevent the influence of a diversity of errors, I have fixed upon the above-mentioned time as a general epoch to which every calculation of the motion of the satellites has been referred, not only in the determination of the periods, but also in every identification.

With the assistance of the analogies that have been given, seven single periods of the revolution of the first satellite were calculated, from the union of which a general compound period has been deduced. The single periods were calculated from a combination of the observation of the 16th of February 1787, with one of the same year, and with six more of the years 1790, 91, 92, 93, 94 and 96.

In the same manner, nine single periods of the second satellite have been calculated by combining the observation of the 16th of February 1787, with one or more of the succeeding, as far as 1797: the observations of 1798 being too close to the node to give a result that might be depended upon.

It will be proper to give the particulars of one of the single periods, to show what degree of accuracy has been used in the calculation.

By observation, the position of the second satellite, February 16, 9^h 38′ 1787, was 3° sf. The situation of the parallel of declination, by analogy (1) was such that the point F, upon the outer circle of Fig. 1. was opposite to 261° 14′ of the inner circle; 3° sf therefore, was at 258° 14′ of the inner circle. By analogy (2), we then find the inclination of the plane of the orbit to the plane of projection, which enables us, by the argument of the satellites being 78° 14′ from the greatest southern elongation, to reduce the apparent place in the circle to the real one, in the orbit elliptically projected. The correction for this reduction will be + 2° 27′; which being applied, gives 260° 41′. But this being the situation which is numbered from the moveable zero, marked 360, it must be brought to its fixed distance from the ascending node by analogy (3); which gives the distance of the zero from that node for this day 104° 25′; and this being added to the former quantity, gives the real place of the satellite in its orbit from the ascending node 5° 6′.

To combine this with the observation of March 28, 10^h 36′ 1797, when the same satellite was 80° 44′ nf; we find that F, in the parallel should now point to 281° 4′ of the inner circle; and that consequently 80° 44′ on the outer circle, will

be opposite to 1° 48′ of the inner circle; and that to reduce it by the inclination of the orbit, a correction of + 15° 41′ must be applied, which gives its situation in the apparent elliptical orbit 17° 41′ from zero. And when the distance of this point from the ascending node, which now is 91° 6′, is added, we have the satellite's real place in its orbit 108° 47′. Then as in 1787, it was at 5° 6′, and is now at 108° 47′, it must have moved over an arch of 103° 41′ of its orbit, to which, if we add 274 revolutions, we find that the sum of its motion amounts to 98743,68 degrees. The interval of time, in which it has moved over this number of degrees, will be found to be 3693,040277 .. days; from this we obtain the required periodical time, which is $13^d\ 11^h\ 8'\ 19''$. This single period differs only 40″ from the compound mean period of the revolution of the second satellite.

The seven detached periods of the first satellite, and the nine of the second have all been calculated in the same manner; and in order to obtain a mean value of them, I judged it proper to allow to the duration of every interval of the time, for which they were calculated, its due weight in the scale, by compounding them together. This was done by adding together the single intervals of time in each period, and also adding together the number of degrees passed over in each single period, and computing then the compound period by these collected sums of times and motions, the result of which is, that the first satellite makes a synodical revolution about the planet in $8^d\ 16^h\ 56'\ 5'',2$, and the second in $13^d\ 11^h\ 8'\ 59''$.

Explanation of the identifying method.

It is evident, that we cannot be satisfied with a conclusion that is drawn from the apparent situation of an observed satellite, if a doubt should remain whether it actually was the satellite which it is said to be; and where such numerous observations are to be examined, a method of identifying the satellites becomes absolutely necessary.

When the periodical revolution is known, the place where a satellite at any given time should be seen, may be strictly calculated; but a method somewhat less rigorous, and much more expedient, will be sufficient for the purpose; but even this will be found to require tedious computations; for in the first place, the motion of the satellite to be identified for any day, must be cast up by the table of its motion in days, hours, and minutes; and for this purpose, the interval of time for which it is calculated, must first be ascertained; this has been done for every day the satellites have been observed. The amount of the motion in orbit being obtained, it must be added to the number of degrees from which the motion proceeded; this at the already mentioned general epoch of 1787, Feb. 16, 9° 38′, was for the first satellite 11° 27′ from the node, and for the second 5° 6′. The sum then will be the real place of the satellite in its orbit.

Now, to obtain the apparent place of a satellite from a given real one, a table must be made, the first column of which must contain the degree of the geocentric longitude of the planet, for which the rest of the columns are calculated. The three analogies that have been given, are to be used for obtaining the contents of the second, third, and fourth columns;

the fifth, contains the natural sine of the inclination given in the fourth column multiplied by 6.

Such a Table has also been calculated for every degree, from three signs 20° to seven signs 19°, which takes in the whole compass of the observations that have been given. I insert the three first lines of the Table as a sample of its construction.

Geor. longitude of the planet.	Position of 360 or zero.	Distance of zero from ditto.	Inclin. of the plane of the orbits to the plane of projection.	Natural sine of the Inclin. × 6.
3ˢ 20°	79° 27′ np	105° 34′	36° 1′	4,85
21	80 17	105 1	36 57	4,79
22	81 5	104 30	37 54	4,73

In order now to use these preparatory calculations, I made an apparatus consisting of a square piece of pasteboard, upon which a circle was drawn and graduated as in figure 1. To the centre of this, I joined a moveable circle also drawn upon pasteboard, and graduated as in the figure. The radius of the inner circle was exactly six inches, and its circumference was nearly in contact with the inside of the outer circle. From what has already been said of the construction of this figure, its use in the identification of the satellites, will easily be understood by a few examples of it.

With the geocentric longitude of the planet taken from the Nautical Almanack, I take out the required quantities from the different columns of the Table. In this operation it might be sufficient to take only the nearest degree for entering the

Table; but as the difference between any two degrees may be had by inspection, I have always used the nearest half degree. For instance, the quantities for the half degree between three signs 21° and 22°, in the second, third, and fifth columns, will be 80° 41′; 104° 45′; and 4,76. And the same quantities will do for any day from March 7, 1787, till April 23, for which day the quantities must be taken from three signs 22°, &c. &c.

Now, suppose it be required to ascertain whether a satellite called the second, which the 15th of March 1787, at 8^h 7′ was observed to be five degrees south following the planet, was indeed the second satellite? Then I see in the general list of calculated motions, that from February 16, 9^h 38′ to March 15, 8^h 7′, is an interval of 26^d 22^h 29′; in which the second satellite has moved 0° 12′ from its place; and as it was then at 5° 6′, it is therefore now 5° 18′ from the ascending node of its orbit.

In using the identifying apparatus, the first thing to be done is, the adjustment of the inner circle to the position it ought to have for the day of observation, which is pointed out by the geocentric longitude of the planet three signs 21½ degrees; the zero must consequently be adjusted to 80° 41′ north preceding; I therefore turn the inner circle upon its centre, till the point 360 is opposite to 80⅔° np; for in the adjustment of the circles to each other, and in reading off the angles pointed out by them, a critical estimation of minutes has not been attempted; whenever, therefore, minutes are given, they must be understood to relate to calculations, or to measures taken with a micrometer.

In the next place, the point of the inner circle, answering

to the calculated situation of the satellite in its orbit, is to be found by the tabular quantity 104° 45′, which is the distance of the ascending node from the zero of that circle; and as the satellite is 5° 18′ from the same node, the quantity given by the Table must be deducted from the same; that is from 365° 18′, and the remainder 260° 33′ will be the place of the satellite on the moveable circle.

Finally, to get the angle of position at which the satellite will be seen, the two ends of a proportional compass must be adjusted to each other, so that when one end of it is opened to six inches, the other may give the quantity in the last column of the Table, which in the present case is 4,76 inches. The distance from 260½° to the transverse must then be measured by the long end of the compass, in a direction parallel to the lines in the figure; and the opening of the short end must be set back again in the same direction, from the transverse towards 260½°; a fine point must be marked with the end of it upon the pasteboard. A black silk thread fixed to the centre of the circle, may then be stretched over the impressed point to intersect the degrees of the outer circle, upon which the positions are reckoned in the order they are marked; and this being done, it will be seen that the intersection of the thread falls upon 2½ degrees of the south following quadrant; which sufficiently identifies the satellite.

In observations that are made when we are in, or very near the node of the orbits of the satellites, their angular positions undergo hardly any change, and can therefore be of no use for identifying them; but they may then be distinguished by their proportional distances from the planet; and these may be very conveniently had in six hundredth parts

of the respective radii of the satellites, and the impression of the fine point whereby the angle of position is obtained will be of eminent use; for by putting one leg of the compass upon this point, and extending the other to the centre of the circle, we shall in the present case have 4,81 inches for the measure of the required distance, which as the radius of the circle is six inches, will be $\frac{481}{600}$ parts of it; and in such parts all the distances which are given in the foregoing observations have been expressed.

I have called this manner of obtaining the angles of position and proportional distances, the identifying method, that it may remain distinguished from strict computation; there is, however, so much real calculation mixed with it, that I may confidently draw the following interesting conclusions from it.

I. With the light of my 20 feet telescope, the first satellite generally becomes invisible at the distance of a little more than half its greatest elongation; I suppose it to be when the identified measure of it is from 302 to about 310.

II. The second satellite becomes invisible at very nearly half the distance of its greatest elongation; I suppose it to be when its identified distance is from 295 to about 305.

III. An interior satellite as large as the first, must be more than half the greatest elongation of the first satellite from the planet; and if it be smaller, it must be at so much greater a distance from the planet, to be seen at its greatest elongation. Nor can there be any chance for seeing it two nights together, when the orbits are contracted by projection.

IV. Exterior satellites that are very faint when at their greatest elongation, can hardly ever be seen at any other time when the orbits are contracted.

V. The first satellite is probably larger than the second; for though the latter is generally the brightest, it seems to be only in consequence of its being farthest from the planet. On comparing the limit of its disappearance with the number 302, expressing that at which the first satellite generally ceases to be visible, we find that the second satellite, upon its own scale, should not be lost in the light of the planet till it came within the limit of 224, instead of 295.

VI. Both the satellites are subject to great variations of light, not owing to the changeable clearness of the air at different times; for by comparing the brightness of one satellite with that of the other when they are seen together, the state of the air will be of equal clearness to both, and yet their comparative brightness has been observed to be very different: for instance, March 14, 1793, the first satellite was brighter than the second, when the distance of the former was to that of the latter as 172 to 235; and February 26, 1798, the first was small, and the second larger when the distance of the former was to that of the latter as 175 to 210.

VII. The variable brightness of the satellites may be owing to a rotation upon their axes, whereby they alternately present different parts of their surfaces to our view. These variations may also arise from their having atmospheres that occasionally hide or expose the dark surface of their bodies, as is the case with the sun, Jupiter and Saturn.

VIII. The real angular distances of the satellites from the planet may be determined from the measures that are given in

the observations, but to enter critically into this subject would extend far beyond the compass of this Paper. The disagreement of the measures is very considerable; this will, however, not appear so remarkable, when the faintness of the satellites is considered, which will not admit of an illumination of the wires of the micrometer. The two measures of the distance of the first satellite that were taken Oct. 11, 1787, and March 12, 1798, should both be considered; but a selection of about six of the most consistent measures of the second satellite, will probably be necessary to give the truest result. For instance, those of 1787, March 18, 20, April 9, and November 9, with those of 1798, March 12 and 19, might be taken. If these measures are brought to their greatest elongation by the identified distances that are given with them, some kind of judgment may be formed of the probable result, when calculation is applied to them.

In my observations I have supposed the distances of the first and second satellites to be 36 and 48 seconds, and by this proportion I have occasionally reduced the identified distances of the two satellites to an equal scale.

IX. The existence of additional satellites has already been considered in a former paper.* Many remarks on them were given under the four heads of interior, intermediate, exterior, and more distant satellites; and, as many additions are contained in the foregoing observations, I shall review the former remarks, with the assistance of the light which the identifying method has thrown upon them, and afterwards, in the same order consider, in each class, what evidence of the existence of such satellites may be derived from the additional observations, especially from those that were made in the year

* Phil. Trans. for 1798, page 59.

1798, when the orbits of the satellites were contracted into a line, which might be examined with greater facility than a more expanded space; and where even the very situation of a star in this given direction, rather than in the numberless others, in which it might be placed, must be a presumption of its being a satellite, provided its distance at the same time should not exceed a certain probable limit.

An interior satellite.

The supposed interior satellite observed January 18, 1790, could not be a star, whose existence was doubtful, as it had light enough for an estimation of its distance in diameters of the planet; its absence, however, not having been noticed the 19th of January, although great attention has been shown to ascertain the sidereal nature of another supposed satellite, observed at the same time with the former, leaves the observation of the eighteenth unsupported.

The observation of the 4th of March 1794, which has been supposed to relate to an interior satellite, is by the identifying method proved to belong to the second satellite; and its observed absence on the 5th from the place where it was the 4th, being thus verified and accounted for, shows that great confidence may be placed on such observations.

The observation of an interior satellite of the 27th of March, 1794, is without a subsequent observation; but then it has already been noticed in remark III, that an interior satellite cannot be seen two successive days, when its orbit is already contracted, as it was on the day of observation.

Addition.

The 15th of February, 1798, an interior satellite was seen about its greatest northern elongation; as it was between the planet and the second satellite (miscalled the fifth), its position must have been 84° 49′ nf. On account of its faintness it was not seen immediately, but as soon as it was perceived, it was surmised to be the second satellite; but the identified distance of the second, which was 455, is much too far for the observed distance of the faint satellite, and proves that in reality the supposed fifth was the second satellite. This is moreover confirmed by its brightness, and by the angle of position which was taken. The first satellite was invisible, its distance being only 124; it even remained invisible the next day, when its distance was 289; the observed faint one, therefore, must have been an interior satellite in a distant part of its northern elongation. The 16th of February, the place where the satellite had been the day before was scrupulously examined in looking for the supposed fifth, and as there was no star remaining in that place, the removal of the interior satellite from its former situation was thereby also ascertained. It has already been noticed that in the contracted position of the orbits an interior satellite observed the 15th could not possibly be seen the 16th, which accounts for its not being noticed the last day of observation. This is one of the cases where the singular situation of a star alone, is almost sufficient to prove it to be a satellite.

The 17th of April, 1801, the interior satellite, which had been seen in its greatest northern elongation, was now seen about its greatest southern elongation. Its situation, by

identification, was 81° sp. The 18th of April, when the stars that were pointed out were looked after, and were all found remaining in their places, no star could be seen where the interior satellite had been situated the 17th; nor could it be expected to be visible, as, by its motion towards the planet, it must already have been involved in the splendour of its light.

An intermediate satellite.

The 26th of March, 1794, an intermediate satellite was seen; by the configuration its distance was greater than that of the first, but less than that of the second. By identification its situation must have been $59\frac{1}{2}$° nf. The 27th of March, the satellite was no longer in the place where it had been seen the 26th, and moreover, a very small star was seen in a place that agreed with what would be the situation of an intermediate satellite, had it accompanied the planet.

An exterior satellite.

The 9th of February, 1790, an exterior satellite was observed. It was by the configuration at double the distance of the second satellite, and by identification, its position was $61\frac{1}{2}$° sf. The observations of two succeeding days proved, that it remained no longer in the place where it had been seen on the 9th.

The 27th of March, 1794, some distant stars south of the planet were observed as being supposed satellites; but they are not sufficiently supported by succeeding observations.

The 5th of March 1796, a star was seen, which the night before had not been in the place where it was at the time of the observation. By the configuration its identified place

must have been 72° np; and its distance exceeded that of the second satellite.

Addition.

The 31st of January, 1791, a satellite in opposition to the second, and at about double the distance from the planet was observed; its identified position was $78\frac{1}{2}°$ np. The 2d of February all the stars of the configuration that had been pointed out, were seen remaining in their places, but the exterior satellite was not among them.

The 26th of February, 1792, a star at double the distance of the first satellite was pointed out, but it has not been accounted for in succeeding observations. By remark IV, however, faint exterior satellites can hardly be expected to be seen at any other time, than when they are about their greatest elongation.

The 11th of February, 1798, an exterior satellite, called the fifth, was observed; its situation was measured 89° 19′,5 nf. The 13th of February, this satellite was no longer in the place where it had been seen the 11th. The 15th of February it was again ascertained that the satellite had left its former situation. The orbits of the satellites, at the time of these observations, were already contracted into a line, and a very faint satellite like this could not remain visible two, and four days successively; its motion, according to remark IV, would in a short time immerge it again into the effusive light of the planet, and render it invisible.

More distant satellites.

The 28th of February, 1794, a small star was seen in a place where the 26th there was none. By the configuration

of that day, and the identifying method, it was at a considerable distance, about 24 degrees north following the planet, and not far from a lettered star which was smaller than the new star. It cannot be supposed that a larger star should have been omitted to have been marked in the situation pointed out by smaller lettered stars, where it must have been seen the 26th, if it had been there.

The 27th of March, 1794, south of the planet, at a considerable distance, were small stars, that had the appearance of satellites; but there are no subsequent observations of them.

The 28th of March, 1797, a distant star is mentioned that was not seen the 25th, although the situation of the lettered stars of that day was carefully examined.

Addition.

The 16th of February, 1798, at $11^h\ 19'$ a very faint satellite, called the sixth, was observed, and from its distance, supposed to be a little before or after its greatest southern elongation. It was so faint, that a small alteration in the clearness of the air, rendered it invisible. On the 18th the sixth satellite was seen again, and, being nearer the planet than it was on the 16th at $11^h\ 19'$, it was supposed to be on its return from the greatest southern elongation. It was also ascertained on the 18th, that it had left the place where it was seen on the 16th. The angle of its position, by a measure taken of it, was $82°\ 55'$ south preceding.

Fig. 1.

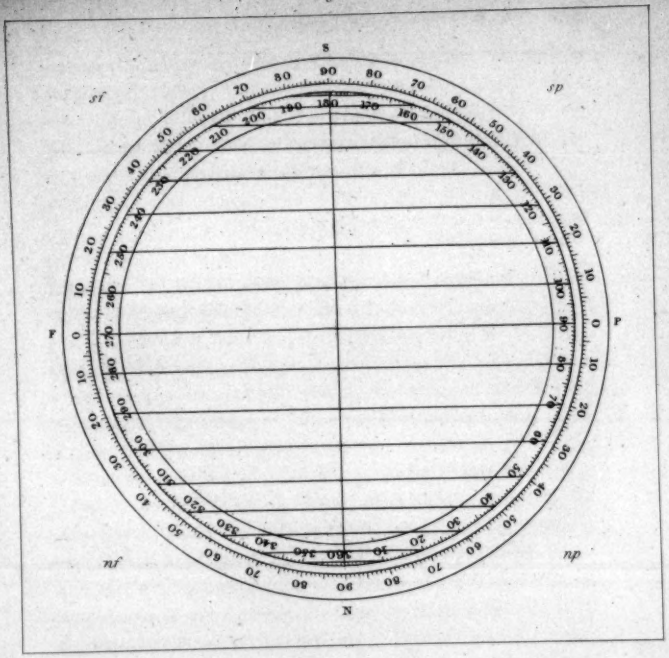


Fig. 2.

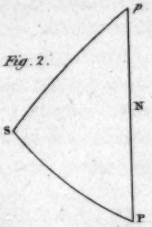
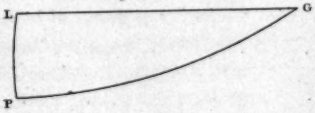


Fig. 3.



XX. *An account of some experiments with a large voltaic battery.* By J. G. Children, *Esq. F. R. S.*

Read June 15, 1815.

In 1809 I presented to the Society a short account of some experiments performed with a voltaic battery of unusually large plates, which has been honoured by publication in the Philosophical Transactions for that year. Since that period I have constructed another of still larger dimensions, the effects of which form the subject of the present communication. The copper and zinc plates of this apparatus are connected together, in the usual order, by leaden straps; they are 6 feet long, by 2 feet 8 inches broad, each plate presenting 32 square feet of surface. All the plates are attached to a strong wooden frame suspended by ropes and pullies, which being balanced by counterpoises, is easily lowered and elevated, so as to immerse the plates in the acid, or raise them out of it, at pleasure. The first trials of the power of this instrument were made in July 1813, in the presence of several philosophical friends, but the effects then fell very short of my expectations, arising, as I afterwards found, from a defect in the construction, which has been since remedied, and another copper plate added to each member of the series, so that every cell now contains one zinc and two copper plates, and each surface of zinc is opposed to a surface of copper. This was done at the suggestion of Dr. Wollaston, and has very

considerably increased the power of the battery. From some comparative experiments, which I made with a small apparatus, the increase in quantity of electricity, thus effected, is at least one half. The cells of the battery are 21 in number, and their united capacities amount to 945 gallons. To each pole of the battery a leaden pipe, about $\frac{1}{4}$ths of an inch in diameter, is attached by solder, and the opposite end of each pipe immersed in a basin of mercury, (a separate basin for each pipe,) by means of which the circuit is compleated, and a perfect contact ensured. The first experiments I shall mention were made on the comparative facility with which different metals are ignited when placed in the electrical circuit. For this purpose, in each experiment, two wires of dissimilar metals were taken, of equal diameter and length; one end of each was in contact with one of the basins of mercury communicating with the poles of the battery, and the other end bent to an angle, and the wires connected continuously by hooking them together. The length of each wire was 8 inches, and the diameter $\frac{1}{36}$th of an inch. The battery was moderately excited by a charge of 1 part acid diluted with 40 parts of water.

Exp. 1. A platina and a gold wire being thus connected, and introduced into the electrical circuit, the platina was instantly ignited, the gold remained unaffected.

Exp. 2. A similar arrangement of gold and silver wires. The gold was ignited, the silver not.

Exp. 3. The same with gold and copper. No perceptible difference in the state of ignition; both metals were heated red.

Exp. 4. Gold and iron. The iron was ignited; the gold unchanged.

Exp. 5. Platina and iron. The iron ignited instantly at the point of contact next the pole of the battery. Then the platina became ignited through its whole extent. After this the iron became more intensely heated than the platina, and the ignition of the latter decreased.

Exp. 6. Platina and zinc. The platina was ignited: the zinc was not; but melted at the point of contact. In a subsequent experiment, the zinc did not melt; but the platina ignited as before.

Exp. 7. Zinc and iron. The iron was ignited: the zinc bore the heat without fusing.

Exp. 8. Lead and platina. The lead fused at the point of contact.

Exp. 9. Tin and platina. The tin fused at the point of contact. No ignition of either wire took place in the two last experiments.

Exp. 10. Zinc and silver. The zinc was ignited before it melted: the silver was not ignited.

The results in every case were the same to whichever pole of the battery either wire was presented. I varied these experiments by introducing several alternations of different wires continuously connected, into the circuit, and obtained in every instance analogous results. Thus

Exp. 11. Alternations of platina and silver, three times repeated: all the platina wires were ignited, and none of the silver.

Exp. 12. One zinc wire between two platina: both the platina wires were ignited, the zinc not.

Exp. 13. One iron between two platina. Both the latter first ignited; then the iron, which soon became most heated, and fused.

It is unnecessary to enter into a farther detail of these experiments; it will be sufficient to say generally, that when wires of several different metals were introduced at once into the circuit, the order of their ignition was precisely that of the former experiments. In one experiment with copper and gold, the copper was decidedly most heated.

I feel some difficulty in attempting an explanation of the preceding phenomena, and offer the following conjecture with diffidence. When a perfect communication is established between the poles of the battery, the electricity circulates without producing any visible effect; but if it meet with resistance in its passage, it manifests itself by chemical action, by the evolution of heat, or both. Thus, if a bar of metal be connected with one pole of the battery, and its extremity immersed in a basin of mercury connected with the other pole, at the instant the surfaces come in contact, heat and light are evolved, which cease as soon as the bar, if it be of sufficient size, is fairly plunged beneath the surface of the quicksilver. If the circuit be completed by two pieces of charcoal, the evolution of heat and light is permanent, as long as their surfaces remain in contact, because that contact can never be so perfect, as to oppose no resistance to the electricity; whereas, in the case of the bar of metal and the mercury, it soon becomes complete, and the current is then uninterrupted. Resistance, therefore, appears to occasion the developement of heat, (whatever be the ultimate cause of the phenomenon,) and as this must be inversely as the conducting power, when any two of the wires connected continuously are placed in the circuit, that which is the worst conductor must be most heated; and thus platina, having the lowest conducting power, is ignited before all the rest; and silver, which

conducts best, is not heated red when connected with any of the other metals. Should it be objected, that if the electricity meet with greater resistance in one body than in the other, equal quantities cannot be transmitted in equal times by the two substances, (a circumstance essential to electrical action,) I answer, that a body may be propelled through two media of different densities, with equal velocity, if the propelling forces be proportionate to the resistances; and it is a necessary consequence that whatever effect the passage of the body be capable of producing in the least resisting medium, it will produce it in a still greater degree in the most resisting; and if that effect be heat, the greatest portion will be developed in the latter instance. In the case in question, indeed, there is but one propelling force; but as it is sufficient to overcome the greater resistance, the analogy is unshaken. That the ignition of the wire is generally first perceptible at the point of contact next the pole of the battery (to whichever pole it be presented) is in favour of the hypothesis. I once thought the phenomena might be owing to the joint effect of difference of conducting power, and inequality of the capacity of different metals for heat; but the experiments of Crawford, Leslie, Dalton, Irvine, and others, militate against that idea; for, according to them, the capacities of iron and platina exceed those of all the other metals, whereas, on the supposition alluded to, they ought to be inferior. From the foregoing results, the order of the conducting powers of the metals tried is silver, zinc, gold, copper, iron, and platina. Tin and lead fuse so immediately at the point of contact, that they cannot be placed. Between gold and copper the difference is trifling; and with regard to platina and iron, their

relations to each other, in this circumstance, seem to be affected by elevation of temperature. It may be observed, that the order of the above metals, as conductors of electricity, nearly follows that of their powers to conduct heat.

In an experiment in which equal lengths of two platina wires, of unequal diameter, (the larger being $\frac{1}{30}$, the smaller $\frac{1}{50}$ of an inch,) were placed together in the circuit *parallel* to each other, the thicker wire was ignited, because it conveyed more electricity without proportional increase of cooling surface. When connected continuously, the order of ignition was reversed. These two results were foreseen by Dr. WOLLASTON, who suggested the experiments.

The experiments which I now proceed to mention, were made with the battery in a high state of excitation; and I consider them as representing nearly the maximum of effect which it is capable of producing. As the quantity of acid was increased from time to time, and that previously added often almost spent before fresh was put in, it is not easy to say exactly what proportion it bore to the water; perhaps the largest may be stated at about $\frac{1}{30}$th. On this, as on former occasions, I found a mixture of nitrous and sulphuric acids, to produce the most powerful and permanent effects.

Exp. 1. 5 ft. 6 in. of platina wire, $\frac{11}{1000}$ of an inch in diameter, were heated red throughout, visible in full daylight.

Exp. 2. 8 ft. 6 in. of platina wire, $\frac{44}{1000}$ of an inch in diameter, were heated red.

Exp. 3. A bar of platina $\frac{1}{5}$ of an inch square, and $2\frac{1}{4}$ inches long, was also heated red, and fused at the end; and,

Exp. 4. a round bar of the same metal, $\frac{314}{1000}$ of an inch

in diameter, and 2¼ inches in length, was heated bright red throughout.

Exp. 5. Fine points of boxwood charcoal intensely ignited in chlorine, neither suffered any change, nor produced any in the gas. The result was similar when heated in azote.

I next tried the power of the battery to fuse several refractory substances. The subject of experiment was placed in a small cavity, made in a piece of well burnt boxwood charcoal, floating on the surface of the mercury in one of the basins before mentioned, and the circuit completed by another piece of charcoal, communicating by stout copper wire, with the other basin.

Exp. 1. Oxide of tungsten, which, (as well as all the other metallic oxides operated on,) had been previously intensely ignited in a charcoal crucible, in a powerful furnace, fused, and was partially reduced. The metal greyish white, heavy, brilliant, and very brittle.

Exp. 2. Oxide of tantalum. A very small portion fused. The grains have a reddish yellow colour, and are extremely brittle.

Exp. 3. Oxide of uranium; fused, but not reduced.

Exp. 4. Oxide of titanium; fused, not reduced. When intensely heated it burnt, throwing off brilliant sparks like iron.

Exp. 5. Oxide of cerium; fused, and when intensely heated it burnt with a large, vivid, white flame, and was partly volatilized, but not reduced. The fused oxide, on exposure for a few hours to the air, fell into a light brown powder, containing numerous shining particles of a silvery lustre,

interspersed amongst it, and exhaled an odour, similar to that of phosphuretted hydrogene.

Exp. 6. Oxide of molybdena; readily fused and reduced. The metal is very brittle, of a steel grey colour, and soon becomes covered with a thin coat of purple oxide.

Exp. 7. Compound ore of iridium and osmium; fused into a globule.

Exp. 8. Pure iridium; fused into an imperfect globule, not quite free from small cavities, and weighing 7.1 grains. The metal is white, very brilliant, and in its present state its specific gravity is 18,68, which must be much too low, on account of the porous state of the globule. In the minutes of the experiments, in July 1813, mention is made of the fusion of a small portion of pure iridium into a globule weighing $\frac{6^2}{100}$ of a grain, which had been previously submitted to the action of a battery of 2000 plates, of four inches square, without melting.

Exp. 9. Ruby and sapphire, were not fused.

Exp. 10. Blue spinel, ran into a slag.

Exp. 11. Gadolinite, fused into a globule.

Exp. 12. Magnesia, was agglutinated.

Exp. 13. Zircon from Norway, was imperfectly fused.

Exp. 14. Quartz, silex, and plumbago, were not affected.

In the year 1796, M. CLOUET converted iron into steel, by cementation with the diamond, with the view of confirming the nature of that substance, and of ascertaining the exact state in which carbon exists in steel. CLOUET had also previously formed steel by cementation with carbonate of lime. Mr. MUSHET repeated this experiment, using instead of the

carbonate, caustic lime, and obtained also what he considered to be cast steel; whence he concluded that the carbon necessary to convert the iron into steel had not been furnished, as CLOUET supposed, by decomposition of the carbonic acid, but that it had found its way from the ignited gas of the furnace to the iron. This result occasioned suspicions of the accuracy of the deductions from the experiment with the diamond; and Mr. MUSHET accordingly, at the suggestion of the editor of the Philosophical Magazine, repeated the experiment made at the Polytechnic School, only *keeping out the diamond*. The results (for he made several experiments) uniformly gave him good cast steel, whence he concludes that we are still without any satisfactory or conclusive proof of the steelification of iron solely by means of the diamond; and adds that he doubts whether the diamond afforded *even one particle of carbon to the iron*. The details of both CLOUET's and MUSHET's experiments, may be found in the fifth volume of the Philosophical Magazine. Sir GEORGE M'KENZIE repeated both CLOUET's experiments and those of Mr. MUSHET, and obtained results confirming the conclusions of the French chemist. The labours of this gentleman indeed seem sufficiently conclusive; but, if a doubt should remain, it occurred to Mr. PEPYS, that the battery would afford an *experimentum crucis* on the subject; and his ingenuity readily suggested a mode of making it, every way unobjectionable. He bent a wire of pure soft iron, so as to form an angle in the middle, in which part he divided it longitudinally, by a fine saw. In the opening so formed, he placed diamond powder, securing it in its situation by two finer wires, laid above and below it, and kept from shifting, by another small wire, bound firmly and

closely round them. All the wires were of pure soft iron, and the part containing the diamond powder, was enveloped by thin leaves of talc. Thus arranged, the apparatus was placed in the electrical circuit, when it soon became red hot, and was kept so for six minutes. The ignition was so far from intense, that few who witnessed the experiment, expected, I believe, any decisive result. On opening the wire, however, Mr. PEPYS found that the whole of the diamond had disappeared; the interior surface of the iron had fused into numerous cavities, notwithstanding the very moderate heat to which it had been exposed; and all that part which had been in contact with the diamond was converted into perfect blistered steel. A portion of it being heated red and plunged into water, became so hard as to resist the file, and to scratch glass. This result is conclusive, for as the contact of any carbonaceous substance, except the included diamond, was effectually guarded against, to that alone can the change produced in the iron be referred. This experiment will also probably be deemed fatal to the opinion of those mineralogists (if any do still maintain that opinion,) who class the diamond with substances of the siliceous genus.

When dry caustic potash was exposed to the intense heat between the two pieces of charcoal, it fused, and appeared to decompose, throwing off a large flame of the peculiar purple red colour, that attends the combustion of potassium. When moist caustic potash was placed in the circuit, the water only was decomposed.

I endeavoured to ascertain if there be any difference in the degree of heat produced at either pole of the battery, by placing two small earthen-ware cups, each containing an equal

weight of mercury, in the circuit, and connected together by a platina wire of such size and length as to be kept constantly ignited. The mercury in the cup connected with the zinc end of the battery, attained in 20 minutes the temperature of 121°; that in the other cup 112°.

The battery, even in its most active state, communicated no charge to the Leyden phial.

I give the following experiment, the last with which I shall occupy the time of the Society, without comment. I separated all the zinc from the copper plates, by dividing the leaden straps that united them; and then by means of other leaden straps, I connected all the zinc plates together as one plate, and all the copper plates in the same manner; thus reducing the whole battery to only two plates, each presenting a surface of 1344 square feet, reckoning the copper surface as only equal to the zinc. When the plates, thus arranged, were suspended, quite out of contact with the acid, a communication was made between the two metallic surfaces by means of a platina wire $\frac{1}{5000}$th of an inch diameter, and about $\frac{1}{30}$th of an inch long, with every possible attention to ensure a perfect contact; but, although the experiment was made in the dark, not the slightest appearance of ignition was perceptible in the minute wire by which these extensive surfaces were connected. It is known, I believe, to almost every member of this society, that Dr. Wollaston has shown, with the delicate apparatus invented by him, that a platina wire, of the same dimensions as that just mentioned, is instantly ignited by a single pair of plates one inch square, on being immersed in a diluted acid. The ratio of the areas of the plates of the respective batteries is as 1 to 48384. When the

plates of the large battery, in the usual order of arrangement, were immersed in mere pump water, previous to any acid having been put into the cells, they ignited $\frac{1}{4}$th of an inch of platina wire $\frac{1}{100}$th of an inch diameter, and fused the end of it into a perfect globule.

XXI. *On the dispersive power of the atmosphere, and its effect on astronomical observations.* *By* Stephen Lee, *Clerk and Librarian to the Royal Society.* *Communicated by* W. H. Wollaston, *M. D. Sec. R. S.*

Read June 15, 1815.

NOTWITHSTANDING the pains which astronomers have taken to determine accurately the refraction of mixed light, nothing, I believe, has ever been done towards ascertaining the dispersive power of common air, or comparative degree of refrangibility of the differently coloured rays in their passage through our atmosphere.

The importance of such an inquiry, however, must be obvious to every one who duly considers the effect which the different degrees of refrangibility of the variously coloured lights must necessarily produce in the apparent situations of differently coloured objects. Stars of different colours must be differently refracted, and the apparent altitude of the sun must vary according to the colour of the dark glass through which he is viewed.

Perhaps this cause alone is sufficient to explain the disagreement which is found to exist between the latitude of a place deduced from observations of circumpolar stars, and that deduced from observations of the sun during the solstices, which has so long occupied the attention of astronomers, and has never yet been satisfactorily accounted for.*

* Vide Mr. PIAZZI's Memoir on the Obliquity of the Ecliptic, in the Memoirs of the Società Italiana, Vol. XI.

The dispersive power of the atmosphere will also show why Aldebaran and the red stars are sometimes seen projected on the moon's disk in occultations by that planet, especially when the immersion or emersion happens to be near her upper limb. For the light of the moon being white, is more refracted than that of the star, and consequently her limb more elevated, which would occasion the star to appear within her disk a few seconds before or after contact.†

The great disagreement which is found to exist in the declination of several of the fixed stars, as given by different observers, may probably be traced to the same cause, stars being more or less refracted according to the predominant colour of which their light is composed.

That the fixed stars differ from each other in respect to the composition of their light, must be obvious to any one who will only take the trouble of comparing them on a fine night. They present a striking variety of colour even to the naked eye. But this difference becomes still more perceptible when they are viewed through a prism properly adapted to the eye-piece of a reflecting telescope.

A star viewed in this manner is converted into a prismatic spectrum. Sirius and the *brilliant* white stars exhibit a large brush of beautiful violet, and the most refrangible colours in great abundance. Aldebaran, α Orionis, and the red stars show only a small proportion of those colours, whilst the dull white stars exhibit a great quantity of intense green light.

† Vide Philosophical Transactions, Vol. LXXXIV. p. 345. Histoire Céleste Françoise, Tome I. p. 393, 403, 413, 425, 428, 467, and Connoissance des Temps for 1817.

The planets also differ much from each other in this respect. The moon, Venus, and Jupiter, seem to possess every colour; but the green is very pale in all of them. Mercury and Mars appear deficient in the middle and most refrangible rays, whilst the light of Saturn seems to be composed principally of the mean rays with a very small proportion of the extreme colours of the prism.*

The different refrangibility of the differently coloured rays is very visible in stars near the horizon. If viewed on a fine night with a power of 200 and upwards, they appear expanded into a prismatic spectrum. Sirius, when within a few degrees of the horizon, presents a most beautiful object.

Having remarked the very oblong figure which the spectrum assumes when near the horizon, and found from repeated observations of different stars that the separation of light begins to be visible as high as 40° or 50° of altitude, I was led to believe that the dispersive power of the atmosphere must be sufficient, in many cases, to produce considerable effect on astronomical observations; and, consequently, to suppose that it would be desirable to ascertain, if possible, the exact degree of separation of the several rays.†

With this view, therefore, I began a series of observations;

* Query. May not this circumstance explain why Saturn, though less brilliant, bears magnifying better than Jupiter and Venus?

† Dr. Herschel, in a note to his Paper on Double Stars, published in the seventy-fifth volume of the Philosophical Transactions, says that the prismatic power of the atmosphere is very visible in low stars; and very justly observes that this power ought not to be overlooked in delicate and low observations: he gives the measure of two diameters of ε Sagittarii, which seem to indicate that the refraction of the extreme rays is about $\frac{1}{77} \pm$, the mean refraction. I think it due to that great astronomer to mention the circumstance, though it was totally unknown to me till long after I had completed my observations on Mars.

the result of which, and the manner of conducting them, I shall now take the liberty of laying before the Society.

The first instrument employed for the purpose with any degree of satisfaction, was the two feet reflector made by Mr. SHORT, and which belongs to the Royal Society. In the compound focus of the eye piece of this telescope, I fixed *horizontally* a narrow slip of ivory. With the instrument thus prepared, I observed Capella, and other low stars near the meridian. By carefully noticing the intervals of time between the first contact and total immersion, and between the first appearance and complete emersion of the star from behind the slip of ivory, I obtained data from which it was easy to calculate its vertical breadth, which, compared by estimation with its horizontal breadth, gave the separation of the extreme rays of light.

It was impossible, however, to remain long satisfied with such coarse measures, and not finding it convenient to go to much expense on this account, I applied to my friend Mr. RENNIE for the loan of his seven feet reflector made by Dr. HERSCHEL, to which I adapted a very excellent wire micrometer made by Mr. TROUGHTON; and thus, by the kind assistance of my friends, I obtained instruments capable of measuring small angles to the fraction of a second of space.

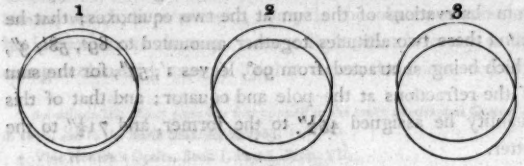
With this apparatus I repeatedly measured the diameter of Mars during his opposition in 1813. The Society's apartments being well situated for the purpose, I observed the planet as soon as he became visible over the buildings, until he attained his meridian altitude, which never exceeded 15°.

With a power of 170 and upwards, the disk of the planet appeared much elongated, especially when near the horizon; the upper limb was of a fine blue, the lower limb of a deep red.

By carefully measuring the diameter of Mars and the breadth of the coloured edges, I endeavoured to ascertain, as exactly as possible, the degree of separation of the differently coloured images of the planet.

But after all it was no easy matter to measure the coloured edges exactly, for the light which was necessary to illuminate the wires, rendered the colours so very faint as to make it extremely difficult to distinguish their precise boundaries. For this reason, and because I wished to apply higher powers than could be used with the micrometer, I adopted the following method, which I found far more convenient, and is, I believe, quite as accurate.

I drew on a sheet of paper several figures of two equal circles cutting each other, placing the centres of the circles in the first figure $\frac{1}{10}$ of their radius from each other; in the second figure $\frac{2}{10}$; in the third $\frac{3}{10}$; and so on. The upper crescent of these figures I painted blue, the lower crescent red, and the part common to both circles of a reddish yellow, softening the colours into each other as they appeared in the planet. For I considered that, in fact, it was not a single image of Mars that was seen, but a number of differently coloured images, lying in the same direction, though lifted one above another, as represented in the annexed figures.



Having prepared a number of these drawings, I repeatedly compared them with the planet viewed through the telescope with different magnifying powers, carefully noting which figure he most resembled, and the time of observation.

This being done, it was easy to calculate the exact altitude from the time of observation, and to make a very near estimate of the separation of the images from the figure referred to, compared with the diameter of the planet found by the micrometer.

From a great number of observations on Mars, Venus, and the fixed stars, taken in all these different ways, I found the deviation of the extreme rays of light to be between $\frac{1}{60}$ and $\frac{1}{70}$ part of the total refraction.

It has already been observed, that the disagreement which is found to exist between the latitude of a place deduced from observations of circumpolar stars, and that from observations of the sun, may perhaps be traced to the use of dark glasses. But this will appear more evidently from a reference to the method employed by Dr. BRADLEY for determining the quantity of refraction, which method is very clearly described by Dr. MASKELYNE in the seventy-seventh volume of the Philosophical Transactions. He says,

That Dr. BRADLEY got the height of the pole from observations of the circumpolar stars, and the height of the equator from observations of the sun at the two equinoxes; that he found these two altitudes together amounted to $89°, 58', 3''$, which being subtracted from $90°$, leaves $1', 57''$, for the sum of the refractions at the pole and equator; and that of this quantity he assigned $45\frac{1}{2}''$ to the former, and $71\frac{1}{2}''$ to the latter.

But Dr. BRADLEY undoubtedly made use of dark glasses for observing the sun, probably smoked glasses, which would give him a pale orange coloured image, or one of less than mean refrangibility; consequently, the quantity of refraction as found by Dr. BRADLEY must be too small for white light. This alone is sufficient to produce a small difference between the results of our observations of the sun and of the stars. I shall now mention two other circumstances which appear to me to have produced a still greater apparent disagreement.

The publication of the Nautical Almanac in 1767, led to the general use of HADLEY's sextant. In the construction of this instrument, coloured glasses were indispensibly necessary; and the great convenience in the use of them over smoked glasses, soon occasioned the application of them to all other instruments. These glasses generally give a deep red image, or one of less refrangibility than smoked glass. The effect of this alteration, therefore, should have been, that arising from too great correction for refraction in every thing depending on observations of the sun.

The introduction of achromatic object glasses * produced an error of a different kind; and one which, in certain cases, tends to correct the other. In the single object glass telescope (and there were no others in BRADLEY's time) the differently coloured images are formed at different focal distances, which, in a manner, compels the observer to adjust his instrument to the most intense light; that is to say, to the orange coloured † image; by this means the fainter colours,

* An achromatic object glass was first applied to the south quadrant at Greenwich in 1772, and to the north quadrant in 1789.

† Vide Newton's Optics, Book I. Part I. Prop. VII.

which occupy the greatest space in the spectrum,* are dissipated, and lost among the more powerful rays. In good achromatic telescopes the case is very different, for all the rays being collected by them into one point, every colour is seen in its proper place, so that the observer, in bisecting the spectrum, takes the altitude of the mean, or the upper extremity of the green image.

But if the upper extremity of the green image be taken in observations of circumpolar stars, a greater correction than Dr. BRADLEY's ought to be applied, in order to get the true height of the pole.

It may not be amiss to observe here, that the observations of Mr. LALANDE at Paris, show a greater disagreement than those at Greenwich; and the observations of Mr. PIAZZI at Palermo, a still greater than those of Mr. LALANDE. This, I apprehend, must arise partly from the lesser elevation of the pole in those places, and partly from the fainter colours in the stellar spectra being more distinctly visible in the clear atmospheres of France and Italy than in England.

It should seem then, that in order to get a perfect knowledge of astronomical refraction, we ought to employ at least three different methods of investigation. 1st. By observations of the fixed stars during the night, when all the prismatic colours are visible. 2dly. By observations of the stars during the day, when none but the orange coloured rays are to be seen. And 3rdly, by observations of the sun with different coloured glasses. By these means we might hope to obtain such an accurate knowledge of atmospheric refraction as would enable us to form tables adapted to every possible circumstance.

* Vide Newton's Optics, Book I. Part II. Prop. III.

But I must not take up the time of the Society by any additional observations. It is in vain for me to pursue the subject any farther, in a situation so ill adapted to astronomical observations as Somerset Place; I shall therefore resign the task to those who are more favourably placed in this respect, and who possess instruments better calculated for an investigation which requires so much accuracy.

XXII. *Determination of the North Polar Distances and proper motion of thirty fixed Stars.* *By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read June 15, 1815.

WHEN a standard catalogue of some of the principal fixed stars was laid before the Society in the year 1813, I ventured to state as my opinion, that the error of this catalogue depending on the mechanical construction of the instrument, did not probably exceed a quarter of a second.

This opinion has been confirmed by the observations of another year; the results of which I have now the honour of transmitting to the Society, as it appears that in those stars which I have continued to observe, I have not had occasion to alter the position of any one, above one-tenth of a second. For this reason I should hardly have thought it necessary to make any farther communication on the subject, had I not wished for an opportunity of adding some valuable deductions respecting the proper motions of these stars.

The comparison of my own catalogue with that of Dr. BRADLEY in the year 1756, is shown in one of the annexed tables, (Table II.) in which the proper motions are given in the last column.

I have also subjoined to my own observations the mean state of the barometer and thermometer, so that the correction may be easily made for any other table of refractions,

as well as that of BRADLEY, which I have employed in reducing the Greenwich observations.

Table III. contains, in addition to the standard catalogue, those stars which have been observed with equal care south of the equator, but from the uncertainty of refraction their positions cannot be so accurately ascertained as those of the former. In this table, the catalogue has been computed both by BRADLEY's and the French Tables of Refraction.

I.

Standard Catalogue of the North Polar Distances of thirty principal fixed Stars, reduced to the beginning of 1813.

	Stars.	No. of Observations in former Catalogue.	Result of one year's obser. N. P. D. January 1, 1813.	Total No. of Observations.	Result of two year's observ. N. P. D. January 1, 1813.	Mean height of barometer.	Mean height of thermometer. In.	Mean height of thermometer. Out.
			° ′ ″		° ′ ″	° ′	° ′	° ′
1	Polaris	167	1 41 21.7	294	1 41 21.66	29 79	49 0	48 0
2	β Urs. min.	90	15 4 48.9	120	15 4 48. 9	29 73	50 7	50 5
3	β Cephei	40	20 15 30.7	70	20 15 30. 6	29 77	49 2	45 5
4	α Urs. maj.	60	27 14 31.5	70	27 14 31. 5	29 81	56 0	55 0
5	α Cephei	40	28 12 12.5	70	28 12 12. 5			
6	α Cassiop.	40	34 29 22.7	70	34 29 22. 6			
7	γ Urs. maj.	48	35 15 55.3	60	35 15 55. 3			
8	γ Draconis	90	38 29 3.6	140	38 29 3. 6			
9	η Urs. maj.	80	39 44 57.9	100	39 44 57. 8			
10	α Persei	40	40 48 52.7	50	40 48 52. 6			
11	Capella	80	44 12 20.5	110	44 12 20. 4			
12	α Cygni	70	45 22 56.9	130	45 22 57. 1	29 77	46 8	43 4
13	α Lyræ	90	51 23 0.5	170	51 23 0. 5	29 82	51 0	49 5
14	Castor	30	57 42 46.7	40	57 42 46. 7	29 81	50 4	48 4
15	Pollux	40	61 31 56.3	50	61 31 56. 4	29 95	50 0	49 2
16	β Tauri	50	61 33 43.7	70	61 33 43. 7	29 92	46 3	44 4
17	α Androm.	35	61 56 29.6	35	61 56 29. 6	29 88	54 2	53 1
18	α Cor. Bor.	80	62 38 55.4	90	62 38 55. 4	29 80	56 8	56 2
19	α Arietis	50	67 25 36.5	80	67 25 36. 5	29 85	43 3	40 3
20	Arcturus	80	69 50 19.1	120	69 50 19. 0	29 82	55 0	54 9
21	Aldebaran	56	73 52 35.4	76	73 52 35. 3	29 91	50 7	50 3
22	β Leonis	20	74 22 57.3	20	74 22 57. 3	29 81	65 4	62 4
23	α Herculis	50	75 23 14.0	50	75 23 14. 0	29 88	57 4	55 9
24	α Pegasi	20	75 47 51.7	30	75 47 51. 6	29 69	48 3	44 4
25	Regulus	65	77 7 22.7	65	77 7 22. 7	29 89	54 7	54 5
26	α Ophiuchi	70	77 17 39.2	90	77 17 39. 1	29 86	56 4	54 3
27	α Aquilæ	80	81 36 58.8	140	81 36 58. 8	29 81	51 0	46 6
28	α Orionis	50	82 38 15.7	60	82 38 15. 7	29 96	53 5	53 3
29	α Serpentis	70	82 58 39.3	70	82 58 39. 3	29 86	58 3	57 3
30	Procyon	40	84 18 14.4	40	84 18 14. 4	29 96	55 4	55 4
31	Polaris SP.					29 79	59 1	52 2

II.

Observations made with the Mural Circle, compared with the observations of Dr. BRADLEY *in the year 1756.*

	Stars.	N. P. D. begin. 1756.	N. P. D. begin. 1814.	Variation in 58 years.	Precession in 58 years.	Difference.	Annual Proper Motion.
		° ′ ″	° ′ ″	′ ″	′ ″	′ ″	″
1	Polaris						
2	β Urs. min.	14 50 47.4	15 5 3.6	+ 14 16.2	14 10.4	0 5.8	+ 0.100
3	β Cephei	20 30 26.2	20 15 15.0	− 15 11.2	15 7.7	3.5	− 0.060
4	α Urs. maj.	26 56 18.5	27 14 50.7	+ 18 32.2	18 27.3	4.9	+ 0.084
5	α Cephei	28 26 27.9	28 11 57.6	− 14 30.3	14 27.3	3.0	− 0.052
6	α Cassiop.	34 48 14.5	34 29 2.9	− 19 11.6	19 11.7	0.1	+ 0.002
7	γ Urs maj.	34 56 57.8	35 16 15.3	+ 19 17.5	19 17.2	0.3	+ 0.005
8	γ Draconis	38 28 22.0	38 29 4.3	+ 0 42.3	0 42.2	0.1	+ 0.002
9	η Urs. maj.	39 27 40.4	39 45 16.1	+ 17 35.7	17 33.9	1.8	+ 0.031
10	α Persei	41 1 47.2	40 48 39.2	− 13 8.0	13 7.6	0.4	− 0.007
11	Capella	44 16 51.5	44 12 15.9	− 4 35.6	4 56.4	20.8	+ 0.358
12	α Cygni	45 34 52.4	45 22 44.3	− 12 8.1	12 2.8	5.3	− 0.091
13	α Lyræ	51 25 47.1	51 22 57.4	− 2 49.7	2 29.9	19.8	− 0.341
14	Castor	57 36 10.7	57 42 53.8	+ 6 43.1	6 40.7	2.4	+ 0.041
15	Pollux	61 24 28.4	61 32 4.3	+ 7 35.9	7 34.1	1.8	+ 0.031
16	β Tauri	61 37 30.9	61 33 39.8	− 3 51.1	3 57.9	6.8	+ 0.117
17	α Androm.	62 15 27.3	61 56 9.6	− 19 17.7	19 20.4	2.7	+ 0.047
18	α Cor. Bor.	62 27 0.2	62 39 7.9	+ 12 7.7	12 4.3	3.4	+ 0.058
19	α Arietis	67 42 12.9	67 25 19.1	− 16 53.8	16 57.4	3.6	+ 0.062
20	Arcturus	69 32 13.6	69 50 38.1	+ 18 24.5	16 30.0	1 54.5	+ 1.972
21	Aldebaran	74 0 15.4	73 52 27.4	− 7 48.0	7 53.9	5.9	+ 0.102
22	β Leonis	74 3 55.6	74 23 17.3	+ 19 21.7	19 15.4	6.3	+ 0.109
23	α Herculis	75 18 46.1	75 23 18.5	+ 4 32.4	4 36.4	4.0	− 0.069
24	α Pegasi	76 6 10.6	75 47 32.3	− 18 38.3	18 32.9	5.4	− 0.093
25	Regulus	76 51 5.1	77 7 40.0	+ 16 35.0	16 37.1	2.1	− 0.036
26	α Ophiuchi	77 14 35.9	77 17 42.3	+ 3 6.8	2 56.9	9.5	+ 0.164
27	α Aquilæ	81 45 27.8	81 36 49.6	− 8 38.2	8 11.4	26.8	− 0.462
28	α Orionis	82 39 42.3	82 38 14.3	− 1 28.0	1 23.3	4.7	− 0.081
29	α Serpentis	82 47 24.7	82 58 51.0	+ 11 26.3	11 31.7	5.4	− 0.093
30	Procyon	84 10 10.3	84 18 21.9	+ 8 11.6	7 14.7	56.9	+ 0.981

The N. P. D. of Polaris determined by upwards of 200 observations of Dr. BRADLEY, by computations made under the direction of Dr. MASKELYNE, a short time before his death, and reduced to the beginning of the year — — — 1749 — 2° 2′ 17″.25

By my observations for — — — — 1813 — 1 41 21 .75

Variation in — — — — — 64 years — 20 55 .50

Precession for — — — — — 64 years — 20 51 .83

Difference — — — — — — — 3 .67

Annual proper motion — — — — — — — — 0.0 57

i. e. The annual precession, which is itself negative, must be increased by the above quantity.

III.

North Polar Distances of forty-four principal Stars for January 1, 1813.

	Stars.	With Bradley's Refraction.	With the French Refraction.	Annual variation.	Annual Proper Motion.
		° ′ ″	° ′ ″	″	″
1	Polaris	1 41 21.6	1 41 21.6	− 19.45	− 0.057
2	β Urs. min.	15 4 49.0	15 4 49.3	+ 14.70	+ 0.100
3	β Cephei	20 15 30.6	20 15 30.9	− 15.70	− 0.060
4	α Urs. maj.	27 14 31.5	27 14 31.9	+ 19.30	+ 0.084
5	α Cephei	28 12 12.5	28 12 12.7	− 14.96	− 0.052
6	α Cassiop.	34 29 22.7	34 29 23.1	− 19.80	+ 0.002
7	γ Urs. maj.	35 15 55.3	35 15 55 8	+ 20.00	+ 0.005
8	γ Draconis	38 29 3.7	38 29 4.2	+ 0.70	+ 0.002
9	η Urs. maj.	39 44 57.9	39 44 58.5	+ 18.20	+ 0.031
10	α Persei	40 48 52.6	40 48 53.2	− 13.50	− 0.007
11	Capella	44 12 20.5	44 12 21.1	− 4.57	+ 0.358
12	α Cygni	45 22 57.0	45 22 57.7	− 12.03	− 0.091
13	α Lyræ	51 23 0.5	51 23 1.2	− 3.00	− 0.341
14	Castor	57 42 46.7	57 42 47.5	+ 7.06	+ 0.041
15	Pollux	61 31 56.4	61 31 57.2	+ 8.00	+ 0.031
16	β Tauri	61 33 43.7	61 33 44.5	− 3.83	+ 0.117
17	α Andromedæ	61 56 29.6	61 56 30.3	− 19.99	+ 0.047
18	α Cor. Bor.	62 38 55.4	62 38 56.2	+ 12.49	+ 0.058
19	α Arietis	67 25 36.5	67 25 37.2	− 17.40	+ 0.062
20	Arcturus	69 50 19.0	69 50 19.8	+ 18.99	+ 1.972
21	Aldebaran	73 52 35.4	73 52 36.3	− 7.95	+ 0.102
22	β Leonis	74 22 57.3	74 22 58.5	+ 20.04	+ 0.109
23	α Herculis	75 23 14.0	75 23 15.1	+ 4.48	− 0.069
24	α Pegasi	75 47 51.6	75 47 52.8	− 19.43	− 0.093
25	γ	75 51 21.0	75 51 22.3	− 20.20	− 0.084
26	Regulus	77 7 22.7	77 7 23.9	+ 17.33	− 0.036
27	α Ophiuchi	77 17 39.1	77 17 40.3	+ 3.10	+ 0.164
28	γ Aquilæ	79 50 0.6	79 50 1.1	− 8.38	− 0.082
29	α	81 36 58.8	81 37 0.0	− 9.06	− 0.462
30	α Orionis	82 38 15.7	82 38 16.9	− 1.37	− 0.081
31	α Serpentis	82 58 39.3	82 58 40.6	+ 11.73	− 0.093
32	β Aquilæ	84 3 4.1	84 3 5.8	− 8.57	+ 0.391
33	Procyon	84 18 14.4	84 18 15.9	+ 7.55	+ 0.981
34	α Ceti	86 39 0.7	86 39 2.6	− 14.75	− 0.005
35	α Aquarii	91 13 21.6	91 13 23.8	− 17.37	− 0.106
36	α Hydræ	97 51 11.3	97 51 13.0	+ 15.19	− 0.066
37	Rigel	98 25 33.8	98 25 36.5	− 4.92	− 0.108
38	Spica Virginis	100 10 51.3	100 10 54.1	+ 18.95	+ 0.002
39	1 α Capricorni	103 4 35.4	103 4 38.5	− 10.80	− 0.083
40	2 α Capricorni	103 6 52.3	103 6 55.5	− 10.80	− 0.090
41	1 α Libræ	105 12 38.7	105 12 42.0	+ 15.20	0.000
42	2 α Libræ	105 15 22.7	105 15 26.2	+ 15.20	+ 0.036
43	Sirius	106 28 0.7	106 28 4.2	+ 4.36	+ 1.158
44	Antares	116 0 16.6	116 0 22.2	+ 8.62	+ 0.012

XXIII. *An essay towards the calculus of functions.* By C. Babbage, *Esq.* *Communicated by W. H. Wollaston, M. D. Sec. R. S.*

Read June 15, 1815.

The term function has long been introduced into analysis with great advantage, for the purpose of designating the result of every operation that can be performed on quantity. This extent of signification has rendered it of essential use, but the various applications of which it admits, and the questions to which it gives rise, do not appear to have met with sufficient attention.

I propose in the following paper to present an outline of a new calculus, which naturally results from it. It comprehends questions of the greatest generality and difficulty, and will probably require the invention of new methods for its improvement.

Many of the calculations with which we are familiar, consist of two parts, a direct, and an inverse; thus, when we consider an exponent of a quantity: to raise any number to a given power, is the direct operation: to extract a given root of any number, is the inverse method. The differential calculus, which is a direct method, naturally gave rise to the integral, which is its inverse: the same remark is applicable to finite differences. In all these cases the inverse method is by far the most difficult, and it might perhaps be added, the most useful.

It is this inverse method with respect to functions, which I at present propose to consider.

If an unknown quantity as x, be given by means of an equation, it becomes a question how to determine its value; similarly if an unknown function as ψ, be given by means of any functional equation, it is required to assign its form. In the first case, it is quantity which is to be determined; in the second, it is the form assumed by quantity, that becomes the subject of investigation. In the one case, the various powers of the unknown quantity enter into the equation; in the other, the different orders of the function are concerned.

Before I proceed, it will be proper to explain the meaning of the order of a functional equation, and likewise to indicate the notation made use of; α, β, γ, &c. are known functional characteristics; ψ, χ, ϕ, are unknown ones.

If in any function as ψx, instead of x, the original function be substituted, it becomes $\psi\psi x$ or $\psi^2 x$: this is called the second function of x. If the process be repeated, the result is $\psi^3 \psi x$ or $\psi^3 x$, the third function of x; and similarly $\psi^n x$, denotes the n^{th} function of x. Suppose

$$\psi x = a + x$$

then $$\psi^2 x = a + a + x = 2a + x$$

and generally $$\psi^n x = na + x$$

A functional equation is said to be of the first order, when it contains only the first function of the unknown quantity; as, for instance,

$$\psi\alpha x + x\psi x - x^2 = 0$$

$$\left(\psi x + \psi\frac{1}{x}\right)^2 - ax + x^2 = 0.$$

If the second function enter, the equation rises to the second order: thus,

$$\psi^{n} x = x$$

$$\psi(x + \psi x) + (\psi x - x)^{n} = o$$

$$\left(\psi^{n} x + \psi \frac{1}{x}\right)^{n} = ax$$

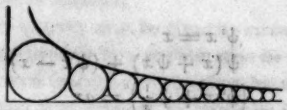
A function of two variables admits of two second functions: thus $\psi(x,y)$ becomes $\psi(\psi(x,y),y)$, and $\psi(x,\psi(x,y))$ or they might be thus expressed $\psi^{2,1}(x,y)$ and $\psi^{1,2}(x,y)$.

These express the second functions; the first taken relative to x, the other relative to y. But besides these two there is another, which arises from taking the second function simultaneously relative to x, and y; it is $\psi\{\psi(x,y),\psi(x,y)\}$. This ought not to be written $\psi^{2,2}(x,y)$ for it is not the second function first taken relative to x and then to y, nor is it the converse of this. In fact, the notation is defective; some method is wanting of indicating the order in which the successive substitutions are made. I shall for the present lay aside the consideration of functional equations, involving more than one variable.

Those of the first order have long been known, but the method in which I have treated them is, I believe, entirely new. Equations of the second and higher orders have never been even mentioned; it is these which present the most interesting speculations, and which are involved in the greatest difficulties. I shall first give some account of the enquiries which led me to this subject, and shall then treat of the various orders of functional equations.

Some few years since, while considering a problem mentioned by PAPPUS, relating to the inscription of a number

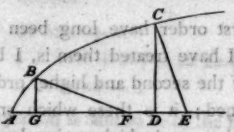
of circles in a semicircle, the following question occurred to me.



If in an hyperbola between its assymptotes a circle be inscribed touching both asymptotes and the curve, and if another circle be inscribed touching the first circle, the curve, and one asymptote, and if this be continued as represented in the figure, what ratio does the area of the circles bear to that of the figure; and conversely, if this ratio is given, what is the nature of the curve? I soon perceived the great difficulty of the subject, and that these and other problems similar to the latter of them, required the application of methods totally different from any with which I was then acquainted.

Hopeless of success, I laid aside the subject until about two years after, when the same difficulty recurred under another form. I had proposed to myself the following problem:

What must be the nature of the curve ABC, such that if



any point C be taken, and the ordinate CD, the normal CE, and subnormal DE be drawn, and if the triangle CDE be turned into such a position that CD may become the base and DE the perpendicular, if DE coincide with some new ordinate as GB, then the normal CE at the first point shall coincide with the normal BF at the second?

This latter question, of far less difficulty than the former, was readily solved, but I did not at first observe that they rested on similar principles; this, however, was pointed out by Mr. HERSCHEL, to whom I had mentioned the subject. Such was the origin of the following enquiries.

The question, in its most general point of view, is the solution of functional equations of all orders.

This, however, is a generality which I do not pretend to have attained. In the first part of this Paper the reader will find a new method of solving all functional equations of the first order; it depends on possessing their particular solutions. In the subsequent part, I have given various methods of solving functional equations of the second and higher orders: some of these possess considerable generality; and if we consider only those in which the n^{th} and inferior orders enter simply, such as

$$\mathrm{F}\left\{x, \psi x, \psi^2 x, \ldots \psi^n x\right\} = 0$$

I have pointed out the means of obtaining their solutions.

The determination of functions from given conditions most probably took its rise from the integration of equations of partial differentials; and we accordingly find that the authors of this calculus were soon engaged in the new problem to which it gave birth. D'ALEMBERT was the first who occupied himself with this subject: he was soon followed by EULER and LAGRANGE; but it is to MONGE that we are indebted for the most general view of the subject. His enquiries were directed to the determination of two functions from given conditions; they are contained in the fifth volume of the *Melanges de Turin*, and in two Papers in the seventh volume of the *Memoires des Savans Etrangers*, 1773.

In the first of these he explains the solution of several functional equations by means of curves of double curvature, and by curve surfaces.

In the second Paper, the question is treated in a more analytical method, and he endeavours to reduce it to the solution of equations of differences. "Je me propose," observes MONGE, "de faire voir que la determination des fonctions arbitraires qui se trouvent dans l'integrale d'une equation aux différences partielles, depend en general, dans les cas que je n'ai pas encore traités, de l'integrale d'une ou de plusieurs équations aux differences finies, dans lesquelles le rapport de la variable principale à sa difference finie est donné soit qu'il soit variable soit qu'il soit constant."

In the same volume is a paper of LAPLACE on this subject, which he views in the same light, and endeavours to reduce functional equations of the first order to those of finite differences. This skilful analyst first solved the functional equation $F\{x, \phi x, \phi\alpha x, \} = 0$. The method he made use of is peculiarly elegant; he converted it into an equation of finite differences in which the difference was constant. Still, however, it appeared by no means the most direct method to make use of such an expedient, nor was it even known that all equations of the first order admitted of its application. This latter objection was, however, removed by Mr. HERSCHEL, who in an excellent paper on functional equations, has extended the method made use of by LAPLACE to the solution of all equations of the first order. His solution is equally elegant and general; it leaves nothing to be regretted, but the narrow limits of our knowledge respecting the integration of equations of finite differences. From this and other causes, I am still

inclined to think that the solution of functional equations must be sought by methods peculiarly their own. There are some other researches on this difficult subject of which I am unable to give any account, from the impossibility of procuring the works in which they are contained; among these is the paper of ARBOGAST, which gained the prize of the Academy of Petersburg in the year 1790.

For the sake of convenience, I shall call any solution of a functional equation which contains one or more arbitrary functions, a general solution; but if the solution of such an equation only contains arbitrary constants, I shall call it a particular solution. With respect to the number of arbitrary functions that may enter into any solution, I shall make some observations at the conclusion of this paper.

PROBLEM I.

Required the general solution of the functional equation,

$$\psi x = \psi \alpha x,$$

supposing we are acquainted with one particular solution.

Let the particular solution be $fx = f\alpha x$; then take $\psi = \varphi f$, φ being an arbitrary function. It is evident that this value of ψ will satisfy the original equation, and that

$$\varphi f x = \varphi f \alpha x$$

is identical, because $fx = f\alpha x$.

Example, let the equation be

$$\psi(x) = \psi(-x)$$

and the particular solution be

$$fx = x^2$$

the general solution is

$$\psi x = \varphi(x^2)$$

which evidently answers the conditions.

As I shall have frequent occasion to make use of symmetrical functions of two or more quantities, I shall for the sake of brevity denote this by putting a line over the functional characteristic; thus $\bar{\varphi}(x,y)$ represents a symmetrical function of x and y, which it is well known possesses the following property,

$$\varphi\{x,y\} = \bar{\varphi}\{y,x\}$$

As we are only considering functional equations of one variable, this will be sufficient for the present purpose; it might perhaps otherwise be more advisable to put the line over the quantities relative to which the function is symmetrical; thus $\varphi\{x,y,\bar{z},\bar{v}\}$ is symmetrical relative to z and v, but it is not so in respect to the other variables. This would possess the advantage of readily designating a function symmetrical relative to two quantities in one way, and likewise symmetrical with respect to two others, but in a different manner,* thus

$$\varphi\left\{\bar{x},\bar{y},\bar{v},\bar{z}^{\frac{1}{2}}\right\}$$

a particular case of this is

$$\frac{v+z+axy}{v^3z^3-a^3x^3y^3}$$

which is symmetrical in one sense relative to x and y, and in a different sense with respect to v and z; but these belong to other enquiries.

PROBLEM II.

Required a general solution of the equation $\psi x = \psi \alpha x$, having given a particular solution of $fx = f\alpha^{x}$

* This is not a mere imaginary refinement; I have constantly had occasion to make use of functions of many variables which were symmetrical by pairs, when investigating the nature of functional equations of more than one variable.

Assume $\psi x = \bar{\varphi}\{fx, \underset{1}{fx}\}$ then it becomes

$$\bar{\varphi}\{fx, \underset{1}{fx}\} = \bar{\varphi}\{f\alpha x, \underset{1}{f\alpha x}\}$$

this equation will be satisfied if we determine f and $\underset{1}{f}$, so that the following equations may be fulfilled.

$$f\alpha x = \underset{1}{f} x \text{ and } \underset{1}{f}\alpha x = fx$$

from these result $f\alpha^2 x = \underset{1}{f}\alpha x = fx$.

but we have by hypothesis a particular solution of $f\alpha^2 x = fx$, therefore the general solution of $\psi x = \psi \alpha x$ is

$$\psi x = \bar{\varphi}\{fx, f\alpha x\}$$

If the function α should be of such a nature that $\alpha^2 x = x$, or even if $\alpha^m x = x$, fx will then become x.

for example, suppose $\psi x = \psi\left(\frac{x}{ax-1}\right)$

then since $\alpha x = \frac{x}{ax-1}$, $\alpha^2 x = \alpha\left(\frac{x}{ax-1}\right) = \frac{\frac{x}{ax-1}}{\frac{x}{ax-1} - 1} = x$

and we have $\psi x = \bar{\varphi}\left\{x, \frac{x}{ax-1}\right\}$

a particular case of this is when $a = o$, $\psi(x) = \psi(-x)$; its solution is

$$\psi x = \bar{\varphi}\{x, -x\} = \varphi(x^2)$$

the same as in the last Problem; as another example take

$$\psi x = \psi \tfrac{1}{x}$$

then since $\alpha x = \frac{1}{x}$, $\alpha^2 x = \frac{1}{\frac{1}{x}} = x$ and $\psi x = \bar{\varphi}\left\{x, \frac{1}{x}\right\}$.

This affords a solution to the following question:

Required the nature of a curve, such that if any two abscissæ, whose rectangle is equal to a given square be taken, their corresponding ordinates may also be equal.

Let $y = \psi x$ be the equation of the curve, also $AN = x$, and if $AN \times AM = a^2$, then the property of the curve is that

$$PN = QM,$$

but $PN = y = \psi x$, and $QM = \psi\,(AM) = \psi\left(\frac{a^2}{x}\right)$ consequently the equation from which ψ must be determined, is

$$\psi x = \psi \frac{a^2}{x}$$

its solutios m

$$y = \psi\, x = \varphi\left\{x, \frac{a^2}{x}\right\},$$

if we make $y = x + \frac{a^2}{x}$, it becomes $yx - x^2 = a^2$, an equation to the hyperbola.

PROBLEM III.

Required the general solution of $\psi x = A\, x \times \psi\, \alpha\, x$; having a particular solution, and also one of $\psi x = \psi\, \alpha^2\, x$.

Assume $\psi\, x = fx \times \bar{\varphi}\left\{\underset{1}{fx}, \underset{2}{fx}\right\}$

making this substitution in the original equation, it becomes $fx \times \bar{\varphi}\left\{\underset{1}{fx}, \underset{2}{fx}\right\} = A\, x \times f\alpha\, x\, \bar{\varphi}\left\{\underset{1}{f\alpha x}, \underset{2}{f\alpha x}\right\}$ make $f_{\alpha}\, x = \underset{2}{fx}$, and $\underset{1}{f\alpha x} = \underset{2}{fx}$, from this it results that $\underset{1}{fx} = \underset{1}{f\alpha^2}\, x$ of which we possess a particular solution, divide both sides by $\bar{\varphi}\left\{\underset{1}{fx}, \underset{1}{f\alpha x}\right\}$ then we have

$$fx = A x \times f\, \alpha x;$$

this is nothing more than the original equation of which, and of $\underset{1}{fx} = \underset{1}{f\alpha^2}\, x$, we have by hypothesis particular solutions:

therefore its general solution is,

$$\psi x = f x \times \varphi\left\{\frac{fx}{f}, \frac{f\alpha x}{f}\right\}$$

The same equation may be solved differently, if we are acquainted with particular solutions of the equations

$$\psi x = \psi \alpha x, \text{ and } \psi x = \mathrm{A} x \times \psi \alpha x.$$

Assume $\psi x = f x \times \varphi x$, it becomes

$$f x \times \varphi x = \mathrm{A} x \times f \alpha x \times \varphi \alpha x,$$

let $f x$ be the particular solution of the original equation, then

$$f x = \mathrm{A} x \times f \alpha x,$$

and by division

$$\varphi x = \varphi \alpha x,$$

but of this also we have given a particular solution, call it $\underset{1}{fx}$, therefore the general expression of ψx, is

$$\psi x = f x \times \varphi \underset{1}{f} x.$$

Let us take for example the equation

$$\psi x = \frac{1-x^2}{1-2x} \psi \frac{x}{x-1}$$

the particular value of ψx is

$$f x = 1 + x,$$

and the particular case of

$$\psi x = \psi \frac{x}{2x-1} \qquad \text{is} \qquad \underset{1}{fx} = \frac{x^2}{x-1},$$

from these considerations the general solution is

$$\psi x = (1 + x)\, \varphi \frac{x^2}{x-1}$$

which will on trial be found to satisfy the equation.*

* Another particular solution of

$$\psi x = \psi \frac{x}{x-1} \text{ is } \psi x = \cos.\log.(x-1)$$

this gives for the general solution of

$$\psi x = \frac{1-x^2}{1-2x} \psi \frac{x}{x-1}$$

$$\psi x = (1 + x)\, \varphi\, (\cos.\log.(x-1).$$

As another example, take

$$\psi x = -\tfrac{1}{x}\psi\tfrac{1}{x}$$

the particular solutions are, that of the equation itself

$$fx = \tfrac{x-1}{x}$$

and that of $\psi x = \psi\frac{1}{x}$ is known to be $\bar{\varphi}\left(x, \frac{1}{x}\right)$, hence the general solution is

$$\psi x = \tfrac{x-1}{x}\bar{\varphi}\left(x, \tfrac{1}{x}\right)$$

which, as is readily seen, fulfills the condition.

PROBLEM IV.

Required the general solution of

$$\psi x = \mathrm{A}\,x \times \psi\alpha x + \mathrm{B}\,x,$$

having given a particular solution fx

Assume $\psi x = fx + \varphi x$, then $\psi\alpha x$ becomes $f\alpha x + \varphi\alpha x$, and the equation is

$$fx + \varphi x = \mathrm{A}\,x \times f\alpha x + \mathrm{A}\,x \times \varphi\alpha x + \mathrm{B}x,$$

subtracting from this the particular solution $fx = \mathrm{A}\,x \times f\alpha x + \mathrm{B}\,x$, which is given by hypothesis, there remains

$$\varphi x = \mathrm{A}\,x \times \varphi\alpha x,$$

this equation may be solved by Prob. III, and we thence obtain the general solution of the given equation.

From this may readily be deduced the solution of the following equations:

$$\psi(\epsilon^y) \pm (\epsilon^{-y}) = \mathrm{A}y$$

suppose f is a particular solution, take $\psi y = fy + \chi y$, the equation becomes

$$f(\epsilon^y) \pm f(\epsilon^{-y}) + \chi(\epsilon^y) \pm \chi(\epsilon^{-y}) = \mathrm{A}y,$$

but we have from the particular solution

$$f(\epsilon^y) \pm f(\epsilon^{-y}) = \mathrm{A}y,$$

this subtracted from the former, leaves

$$\chi(\epsilon^{y}) \pm \chi(\epsilon^{-y}) = 0.$$

Let us first consider the upper sign, then a particular solution of

$$\chi(\epsilon^{y}) = -\chi(\epsilon^{-y}),$$

$$\text{is } \chi(y) = (\log. y)^{2n+1}.$$

If we take the lower sign, then a solution of

$$\chi(\epsilon^{y}) = \chi(\epsilon^{-y})$$

$$\text{is } \chi(y) = (\log. y)^{2n}.$$

From these considerations it appears, that the general solutions of the given equations are

$$\psi y = fy + \phi\left\{(\log. y)^{2n+1}\right\}$$

$$\text{and } \psi y = fy + \phi\left\{(\log. y)^{2n}\right\}$$

according as the upper or under sign is used.

The equation just solved was not constructed as an example to this particular rule, but is selected because it has actually occurred. It is used by Mr. Herschel in the Philosophical Transactions, 1814, for the purpose of assigning the sums of several very curious series. He there observes, that when the upper sign is used $^{2n}\mathrm{L}(1+y)$, and when the lower takes place $^{2n+1}\mathrm{L}(1+y$, are particular solutions, these may therefore be generalized by the introduction of an arbitrary function

If $\psi\epsilon^{y} + \psi\epsilon^{-y} =$ a rational function of y containing only even powers,

or if $\psi\epsilon^{y} - \psi\epsilon^{-y} =$ a rational function of y containing only odd powers,

they admit of the following solutions, in the first case, let

$$\psi\epsilon^{y} + \psi\epsilon^{-y} = a_0 + a_1 y^{2} + a_2 y^{4} + \&c. + a_n y^{2n}$$

assume $\psi y = \underset{0}{A} + \underset{1}{A}(\log.y)^1 + \&c. + \underset{n}{A}(\log.y)^n$

ψe^y becomes $\underset{0}{A} + \underset{1}{A}y^1 + \underset{2}{A}y^2 + \&c. + \underset{n}{A}y^n$

ψe^{-y} becomes $\underset{0}{A} + \underset{1}{A}y^{-1} + \underset{2}{A}y^{-2} + \&c. + \underset{n}{A}y^{-n}$

By comparing the co-efficients,

$$\underset{0}{A} = \tfrac{1}{2}a \quad \underset{1}{A} = \tfrac{1}{2}a, \quad \&c. \quad \underset{n}{A} = \tfrac{1}{2}a$$

and calling the right side of the equation $F(y^n)$, if

$$\psi e^y + \psi e^{-y} = Fy^n,$$

a particular solution is

$$\psi y = \tfrac{1}{2}F\left\{(\log.y)^n\right\}$$

and similarly if

$\psi e^y - \psi e^{-y} = F(y) =$ a function containing only odd powers of y, one particular solution is

$$\psi y = \tfrac{1}{2}F\left\{\log.y\right\}$$

and the general solutions may be readily deduced as above.

PROBLEM V.

To reduce the equation $\psi x + Ax \times \psi \alpha x + \&c. + Nx \times \psi \nu x + X = 0$, to one in which the last term is wanting, by means of a particular solution,

Let fx be the given solution, make $\psi x = fx + \varphi x$, and substituting this value, the equation becomes

$$\left.\begin{array}{l} fx + Ax \times f\alpha x + Bx \times f\beta x + \&c. + Nx \times f\nu x + X \\ +\varphi x + Ax \times \varphi\alpha x + Bx \times \varphi\beta x + \&c. + Nx \times \varphi\nu x \end{array}\right\} = 0$$

the upper line is by hypothesis equal to nothing, therefore the equation is reduced to this,

$$\varphi x + Ax \times \varphi\alpha x + \&c. + Nx \times \varphi\nu x = 0 \qquad (1)$$

and if we can discover the general solution of this latter equation, that of the former may be readily found.

Supposing α, β, γ, &c. ν, were to become $\alpha, \alpha^2, \alpha^3, \ldots \alpha^n$, the equation (1) would be changed into

$$\varphi x + \mathrm{A}x \times \varphi\alpha x + \mathrm{B}x \times \varphi\alpha^2 x + \&c. + \mathrm{N}x \times \varphi\alpha^{n-1} x = 0 \quad (2)$$

after further if αx, is such a function of x, that $\alpha^n x = x$, if we are acquainted with one particular solution of (2), we may easily determine the general one thus:

Assume $\varphi x = fx \times \overline{\chi}\left\{x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x\right\}$

here we must observe, that since χ is symmetrical relative to all the quantities contained within the brackets, it is immaterial in what order they are placed, and from the condition that $\alpha^n x = x$, it follows that if we substitute $\alpha^k x$ for x, (k being successively equal to 1, 2, 3, 4, and $n-1$, we shall always have these values $x, \alpha x, \alpha^2 x \ldots \alpha^{n-1} x$ only differently arranged, from these considerations the equation (2) will become

$$0 = (fx + \mathrm{A}x \times f\alpha x + \mathrm{B}x \times f\alpha^2 x + \&c. + \mathrm{N}x \times f\alpha^{n-1} x)$$
$$\overline{\chi}\left\{x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x\right\};$$

this equation may be satisfied by making the factor which multiplies $\overline{\chi}$ equal to nothing, and this is always the case when f is a particular solution, hence

$$\varphi x = fx \times \overline{\chi}\left\{x, \alpha x, \alpha^2 x, \ldots \alpha^{n-1} x\right\}.$$

Problem VI.

To find a function of x, such that if instead of x we successively substitute $\alpha x, \beta x, \gamma x$, &c. νx, the results shall all be equal to the original function; or in other words, to determine φx from the equations

$$\varphi x = \varphi\alpha x = \varphi\beta x = \&c. = \varphi\nu x$$

find f, so as to satisfy the equation

$$f x = f_{\alpha} x \qquad (a)$$

take any particular value

Find f, so as to satisfy the equation

$$f_1 f x = f_1 f_{\alpha} \beta x = f_1 f \beta x \qquad (b)$$

for it is known from (a) that

$$f = f_{\alpha}$$

take some particular value, and determine f from the equation

$$f_2 f_1 f x = f_2 f_1 f \beta \gamma x = f_2 f_1 f \gamma x,$$

continue this as far as

$$f_n f_{n-1} \ldots f x = f_n f_{n-1} \ldots f_{\nu} x \qquad (n)$$

then ϕ being any arbitrary function

$$\phi \left\{ f_n f_{n-1} \ldots f_1 f x \right\}$$

will satisfy the conditions of the problem.

As an example, let it be required to find a function which shall not change, when for x we substitute x, $-x$, or $\sqrt{\frac{x}{x^2-1}}$

here $\alpha x = -x$, and $\beta x = \sqrt{\frac{x}{x^2-1}}$

(a) becomes $f(x) = f(-x)$,

hence $fx = \bar{\phi} \left\{ x, -x \right\}$ as a particular case, take $fx = x^2$, then (b) becomes

$$f_1 x^2 = f_1 (\beta x)^2 = f_1 \frac{x^2}{x^2-1}$$

whose solution is $f_1 x = \bar{\phi} \left\{ x, \frac{x}{x-1} \right\}$

take the case of $f_1 x = \frac{x^2}{x-1}$

then we find

$$\varphi \underset{1}{f} f x = \varphi \left\{ \frac{x^2}{x^2 - 1} \right\}$$

which fulfils the given conditions.

In the same manner it may be found, that the function

$$\varphi \left\{ \frac{1 - ax^2 + x^4}{1 + ax^2 + x^4} \right\}$$

will remain the same, whether the variable is x, $-x$, or $\frac{1}{x}$.

PROBLEM VII.

Given any two series of functions αx, βx, &c. νx and $\underset{1}{\alpha} x$, $\underset{1}{\beta} x$, &c. $\underset{1}{\nu} x$, required the form of ψ, so that the following equations may be fulfilled,

$$\psi \alpha x = \underset{1}{\psi} \alpha x$$

$$\psi \beta x = \underset{1}{\psi} \beta x$$

&c. &c.

$$\psi \nu x = \underset{1}{\psi} \nu x.$$

Determine f from the equation

$$f \alpha x = \underset{1}{f} \alpha x \qquad (a)$$

take some particular case, and determine $\underset{1}{f}$ from the equation

$$f f \alpha \beta x = \underset{1}{f} \underset{1}{f} \alpha \beta x = \underset{1}{f} f \alpha \beta x = f \underset{1}{f} \alpha \beta x \qquad (b)$$

take a particular case and find $\underset{2}{f}$ from the equation

$$f f f \alpha \beta \gamma x = \underset{2}{f} \underset{1}{f} \underset{1}{f} \alpha \beta \gamma x = \underset{2}{f} \underset{1}{f} f \alpha \beta \gamma x = \underset{2}{f} f \underset{1}{f} \alpha \beta \gamma x \qquad (c)$$

and continue this to

$$\underset{n}{f} \underset{n-1}{f} \ldots f \alpha \beta \gamma \ldots \nu x = \underset{n}{f} \underset{n-1}{f} \ldots f \alpha \beta \gamma \ldots \underset{1}{\nu} x \qquad (n)$$

Then will

$$\psi x = \underset{n\,n-1}{\phi f f \ldots f} \alpha \beta \gamma \ldots \nu x$$

satisfy the conditions.

We now possess the means of solving a much more general problem than we have yet attempted, it is the general solution of any functional equation of the first order.

PROBLEM VIII.

Required the general solution of the equation

$$F\left\{x, \psi x, \psi \alpha x, \ldots \psi \nu x\right\}$$

having given a particular one, containing one or more arbitrary constants.

Find from Problem VI. an arbitrary function, such that it shall remain the same, when αx, βx, &c. νx, are substituted for x, call this ϕx, and let the particular solution be

$$\psi x = f\left\{x, a, b, c, \&c.\right\}$$

As these arbitrary constants are supposed not to be contained in the given equation $F\left\{x, \psi x, \psi \alpha x, \ldots \psi \nu x\right\} = o$, when the particular solution is substituted, they must destroy each other, whatever be their value.

If therefore, instead of a, b, c, &c. we put $\phi x, \phi x, \phi x$, &c. (which are arbitrary functions fulfilling this condition, viz. that they remain identical when αx, βx, &c. are pu for x:) it is evident that these arbitrary functions must also destroy each other, therefore

$$\psi x = f\left\{x, \underset{1}{\phi x}, \underset{2}{\phi x}, \phi x, \&c.\right\}$$

is a general solution containing as many arbitrary functions as the particular solution did constant quantities

Examples, let

$$x\psi x = \psi\tfrac{1}{x}$$

a particular solution is $\psi x = a\frac{x+1}{x}$

and $\varphi\left\{x, \frac{1}{x}\right\}$ does not change when x becomes $\frac{1}{x}$, therefore the general solution is

$$\psi x = \frac{x+1}{x}\varphi\left(x, \tfrac{1}{x}\right)$$

take $\psi x - \psi\frac{1}{x} = a(1 - x^2)$

a particular case of this equation is

$$\psi x = a + bx$$

therefore, the general solution is

$$\psi x = a + \bar{\varphi}\left\{x, \tfrac{1}{x}\right\}.$$

Let $\psi(x) \times \psi(-x) = \frac{1+x}{1-x}\left(\frac{1}{x}\psi\frac{1}{x}\right)^2$

as a particular solution take

$$\psi x = \tfrac{1-x}{ax}$$

and $\varphi\left\{\frac{1 - ax^2 + x^4}{1 + ax^2 + x^4}\right\}$ is a functional equation which does not change when x becomes $-x$ or $\frac{1}{x}$, therefore the general solution is

$$\psi x = \frac{1 - x}{x\varphi\left\{\frac{1 - ax^2 + x^4}{1 + ax^2 + x^4}\right\}}$$

φ being arbitrary.

Given the equation

$$2\psi\left(\tfrac{x}{\sqrt{x^2-1}}\right) = \psi x + \psi(-x) + \tfrac{\psi x - \psi(-x)}{\sqrt{x^2-1}}$$

a particular solution is

$$\psi x = a + bx$$

hence the general solution will be found to be

$$\psi x = \varphi\left(\tfrac{x^2}{x^2-1}\right) + x\varphi\left(\tfrac{x^2}{x^2-1}\right)$$

On the number of arbitrary functions introduced into the complete solution of a functional equation.

When from a functional equation of the first order, we determine the form of the unknown function, one or more constant quantities are generally introduced; these as I have shown in a preceding Problem, may be changed into arbitrary functions of the unknown quantity which fulfil certain prescribed conditions.[1]

A question naturally arises as to the number of these arbitrary functions, and how many any given equation admits of in its most general solution.

The train of reasoning usually made use of to prove, that a differential equation of the n^{th} order, requires in its complete integral n, arbitrary constants may be pursued on the present occasion, though from several reasons, it would perhaps be desirable to have a proof resting on a different principle; as I have not been successful in discovering any other, I shall give the only one I am at present possessed of.

Let $\psi\, x = \mathrm{F}\left\{ x, \underset{1}{a}, \underset{2}{a}, \ldots \underset{n}{a} \right\}$

for x, put any number of known functions, as αx, βx, .. νx, the results will be

$$\psi\, x = \mathrm{F}\left\{ x, \underset{1}{a}, \underset{2}{a}, \ldots \underset{n}{a} \right\} \qquad (0)$$

$$\psi\, \alpha x = \mathrm{F}\left\{ \alpha x, \underset{1}{a}, \underset{2}{a}, \ldots \underset{n}{a} \right\} \qquad (1)$$

$$\psi\, \beta x = \mathrm{F}\left\{ \beta x, \underset{1}{a}, \underset{2}{a}, \ldots \underset{n}{a} \right\} \qquad (2)$$

&c. &c.

$$\psi\, \nu x = \mathrm{F}\left\{ \nu x, \underset{1}{a}, \underset{2}{a}, \ldots \underset{n}{a} \right\} \qquad (n)$$

From these $n+1$ equations we may eliminate the n arbitrary constants, and the resulting equation will be of the form

$$o = \underset{1}{\mathrm{F}}\left\{x, \psi x, \psi_{\alpha} x, \ldots \psi_{\nu} x\right\} \qquad (\mathrm{A})$$

In arriving at this equation, we have eliminated n arbitrary constants, and therefore it might possibly be inferred that the general solution of (A) is

$$\psi x = \mathrm{F}\left\{x, \underset{1}{a}, \underset{2}{a}, \ldots \underset{n}{a}\right\}.$$

But this is too hasty a conclusion, for it is evident, that we should equally have arrived at equation (A), if each of the constant quantities in (*o*) had been changed into a function of x so constituted that it should not alter by the substitution of αx, βx, &c. νx.

It would now appear, that putting such values for the constant quantities, the result would be the general solution of (A).

This reasoning is certainly plausible, and such a solution is undoubtedly a very general one; still, however, there are reasons which incline me to believe, that other solutions exist of a yet more general nature.

On functional equations of the second and higher orders.

When we consider functional equations of an order superior to the first, new difficulties present themselves; the artifices which were used with success in the preceding part of this paper, are no longer of any avail.

Those which we have now to consider seem to possess an entirely distinct character.

PROBLEM IX.

Required the solution of the equation

$$\psi^{2} x = x \qquad (a)$$

Subtract ψx from both sides, then we have

$$\psi^{2} x - \psi x = -\psi x + x = -(\psi x - x)$$

consequently,

$$\Delta \psi x = -\Delta x \qquad (b)$$

again multiply (a) by ψx, then we have

$$\psi^{2} x \times \psi x = \psi x \times x$$

From this we learn, that (b) must be integrated on the hypothesis of $x \psi x$ being constant, hence

$$\psi x = -x + c = -x + f(x \psi x)$$

f being an arbitrary function

ψ is determined from the equation

$$\psi x + x - f(x \psi x) = 0.$$

For f we may put $f^{-1} f$, and it becomes

$$f(x + \psi x) - \underset{1}{f}(x \psi x) = o \qquad (c)$$

A friend to whose valuable remarks on this subject, I am much indebted, has communicated to me the following method of obtaining the same solution. Assume any symmetrical function of x and v.

$$\tilde{\varphi}(x, v) = o$$

then from the nature of the equation we have the two following equations $x = \psi v$ and $v = \psi x$ consequently

$$x = \psi v = \psi \psi x = \psi^{2} x,$$

hence ψx may be found from the equation

$$\tilde{\varphi}\{x, \psi x\} = o$$

This at first sight appears different from (c), but it is not so

in reality, for it may easily be shown, that from the sum and product of two quantities any symmetrical function may be composed.

As particular cases of the equation

$$\psi^2 x = x$$

we may notice

$$\psi x = a - x, \quad \frac{x}{ax-1}, \quad \frac{b-x}{1-ax}, \text{ and } \frac{ax + \sqrt{a^2 + a^2 - 4x^2}}{2}$$

Problem X.

Required the solution of

$$\psi^2 x = x$$

another solution may be found from the following principle.

Assume $$\psi x = \overline{\varphi}^1 f \varphi x$$

hence $$\psi^2 x = \overline{\varphi}^1 f \varphi \overline{\varphi}^1 f \varphi x = \overline{\varphi}^1 f^2 \varphi x$$

or $$\overline{\varphi}^1 f^2 \varphi x = x.$$

Now this equation can be satisfied if we are acquainted with a particular solution of $f^2 x = x$, this may be found from the last problem, and since $f^2 x = x$, the equation becomes

$$\overline{\varphi}^1 \varphi x = x$$

which is identical, consequently

$$\psi x = \overline{\varphi}^1 f \varphi x$$

some particular cases are

$$\psi x = \overline{\varphi}^1 (a - \varphi x), \quad \overline{\varphi}^1 \left(\frac{\varphi x}{a \varphi x - 1}\right), \quad \overline{\varphi}^1 \left(\frac{a}{\varphi x}\right) \text{ &c.}$$

from each of these by assigning particular values to φ, new values of f may be determined, and these in their turn will furnish new forms of the function ψx.

Some time ago I received from the gentleman already alluded to, the following solution of the equation

$$\psi^2 x = x$$

$$\psi x = \varphi(-\varphi x)$$

and likewise the solution $\varphi^{-1}\left((-1)^{\frac{1}{n}}\varphi x\right)$ of the equation $\psi^n x = x$; this first led me to the substitution of $\varphi^{-1} f \varphi x$, which is of such essential use in these enquiries.

PROBLEM XI.

Given the equation.

$$\psi^n x = x.$$

Assume as before $\psi x = \varphi^{-1} f \varphi x$,

then $$\psi^2 x = \varphi^{-1} f \varphi \varphi^{-1} f \varphi x = \varphi^{-1} f^2 \varphi x$$

$$\psi^3 x = \varphi^{-1} f^2 \varphi \varphi^{-1} f \varphi x = \varphi^{-1} f^3 \varphi x,$$

and generally $\psi^n x = \varphi^{-1} f^n \varphi x$,

hence our equation becomes

$$\varphi^{-1} f^n \varphi x = x. \qquad (a)$$

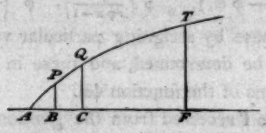
Suppose we have one particular solution of the equation, substitute this instead of f, and the equation (a) becomes identical, whence

$$\psi x = \varphi^{-1} f \varphi x$$

and from this other values of f may be determined, and so on ad infinitum.

The equation we have just considered, affords a ready solution of the following Problem.

Required the nature of a curve, such that taking any point B in the abscissa, and drawing the ordinate BP



if we make AC another abscissa equal to BP the preceding ordinate, and if we continue this n times, then the n^{th} ordinate may be equal to the first abscissa.

If $AB = x$ and the equation of the curve be $y = \psi x$.

$PB = y = \psi x$ and $AC = PB = \psi x$,

and $QC = \psi\psi x = \psi^2 x$, and generally the n^{th} ordinate TF is equal to $\psi^n x$, hence

$$\psi^n x = x$$

which is the equation whose solution has been just found.

Problem XII.

Given the equation

$$\psi^2 x = \alpha x$$

required the form of ψ.

Assume $\psi x = \bar{\phi} f \phi x$

then $\psi^2 x = \bar{\phi} f^2 \phi x$

and $\bar{\phi} f^2 \phi x = \alpha x$

take the function ϕ on both sides, then this becomes

$$f^2 \phi x = \phi \alpha x.$$

This is a functional equation of the first order relative to ϕ, and may be solved either by the methods exhibited in the first part of this paper, or by the very elegant one of Laplace, f is a perfectly arbitrary function, except that neither fx nor $f^2 x$ must be equal to x: from not attending to this circumstance, I was at first led into several errors; the reason of these two restrictions is, that in the first case we at once determine ψx to be equal to x, and in the second, we in fact make $\alpha x = x$, neither of which are necessarily true.

Problem XIII.

Given the equation.

$$\psi^n x = \alpha x$$

This admits of a solution similar to the last, by assuming ψx

equal to $\overset{-1}{\varphi} f \varphi x$, we find

$$\overset{-1}{\varphi} f^n \varphi x = ax$$

and by taking φ on both sides, it becomes

$$f^n \varphi x = \varphi a x$$

give f some particular value and determine φ as in the last problem.

PROBLEM XIV.

Given the equation

$$\psi a \psi \beta x = \gamma x$$

Assume $\psi x = \overset{-1}{\varphi} f \varphi f_1 x$

hence $\psi a \psi \beta x = \overset{-1}{\varphi} f \varphi f_1 a \overset{-1}{\varphi} f \varphi f_1 \beta x$

make $f_1 = \overset{-1}{a}$, then the equation becomes

$$\overset{-1}{\varphi} f \varphi \overset{-1}{\varphi} f \varphi \overset{-1}{a} \beta x = \overset{-1}{\varphi} f^2 \varphi \overset{-1}{a} \beta x = \gamma x$$

and by taking φ on both sides

$$f^2 \varphi \overset{-1}{a} \beta x = \varphi \gamma x$$

this is an equation of the first order relative to φ.

The equation $\psi a \psi a \ldots (n)\ \psi \beta y = \gamma y$ might be solved in the same manner, but they are both reducible by a simple transformation to the form

$$\psi^n y = \mathrm{F} y$$

PROBLEM XIV.

Another method of solving the equation

$$\psi^2 x = x$$

Assume $\psi x = \overset{-1}{\varphi} f \varphi x$, then $\psi^2 x = \overset{-1}{\varphi} f^2 \varphi x$, and we have

$$\overset{-1}{\varphi} f^2 \varphi x = x.$$

Let $y = \varphi x$, then $x = \overset{-1}{\varphi} y$ and the equation becomes

$$\overset{-1}{\varphi} f^2 y = \overset{-1}{\varphi} y$$

an equation of the first order from which y may be found or thus assume

$$\bar{\varphi}^{1} y = \bar{\chi}\left\{y, f^{n} y\right\}$$

and make $f^{n} y = y$,

this method is much more extensive in its application than any of those before it.

PROBLEM XVI.

Required the solution of

$$\psi^{n} x = x$$

the same method applies equally in this case,

Assume $\psi x = \bar{\varphi}^{1} f \varphi x$

then $\bar{\varphi}^{1} f^{n} \varphi x = x$

for x put $\bar{\varphi}^{1} x$ it becomes

$$\bar{\varphi}^{1} f^{n} x = \bar{\varphi}^{1} x.$$

Take $\bar{\varphi}^{1}$ an arbitrary symmetrical function of

$$x, f^{n} x, f^{2n} x, \text{ and } f^{kn} x,$$

then $\bar{\varphi}^{1} x = \chi\left\{x, f^{n} x, f^{2n} x, \ldots f^{kn} x\right\}$

and determine f such that

$$f^{(k+1)n} x = x$$

a particular solution is sufficient, and it is evident, this value of $\bar{\varphi}^{1}$ will satisfy the equation.

PROBLEM XVII.

Given the equation

$$\psi\alpha(y, \psi y) = y$$

required the form of y.

Assume $\psi y = \bar{\varphi}^{1} f \varphi y$, then it becomes

$$\bar{\varphi}^{1} f \varphi \alpha (y, \bar{\varphi}^{1} f \varphi y) = y,$$

take successively on each side the functions φ, f^{-1}, and $\vec{\varphi}$, the equation becomes

$$\alpha(y, \vec{\varphi}\, f \varphi y) = \vec{\varphi}\, f^{-1} \varphi y$$

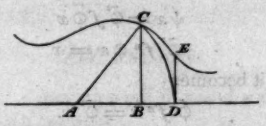
for y put $\vec{\varphi}\, y$, then

$$\alpha(\vec{\varphi}\, y, \vec{\varphi}\, f y) = \vec{\varphi}\, f^{-1} y,$$

this is a functional equation of the first order relative to $\vec{\varphi}$, give f any determinate value and solve the equation.

From hence we may deduce the solution of the following question.

Required the nature of a curve



such that taking any abscissa AB and drawing the ordinate CB, if with centre A and radius AC, we describe a circle cutting the abscissa in D, the ordinate ED may be equal to the first abscissa AB.

Let $y = \psi x = \mathrm{CB}$

$$\mathrm{AD} = \mathrm{CA} = \sqrt{x^2 + (\psi x)^2}$$

and $\mathrm{ED} = \psi(\mathrm{AD})$, hence

$$x = \psi\left(\sqrt{x^2 + (\psi x)^2}\right)$$

which is a particular case of the preceding problem.

PROBLEM XVIII.

To reduce the equation

$$\mathrm{F}\left(x, \psi x, \psi^2 \alpha x, \ldots \psi^n{}_\gamma x\right) = 0$$

to one of the form of

$$F\left(x, \psi x, \psi^2 x, \ldots \psi^n x\right) = 0.$$

Assume $\psi x = \overline{A}^{1} \varphi A x$, the equation by this substitution becomes

$$F\left\{x, \overline{A}^{1} \varphi A x, \overline{A}^{2} \varphi^2 A x, \ldots \overline{A}^{n} \varphi^n A x\right\} = 0$$

find by Problem VI. such a value Ax that it shall not change by the substitution of $\alpha x, \beta x, \gamma x$, &c. νx, put for Ax the quantity y, and the equation becomes

$$F\left\{\overline{A}^{1} y, \overline{A}^{1} \varphi y, \overline{A}^{2} \varphi^2 y, \ldots \overline{A}^{n} \varphi^n y\right\} = 0$$

which is an equation of the required form.

Problem XIX.

Required the solution of the equation

$$F\left\{x, \psi x, \psi^2 x, \ldots \psi^n x\right\} = 0$$

Assume $\psi x = \overline{\varphi}^{1} f \varphi x$, then $\psi^n x = \overline{\varphi}^{1} f^n \varphi x$, and the equation becomes

$$F\left\{x, \overline{\varphi}^{1} f \varphi x, \overline{\varphi}^{1} f^2 \varphi x, \ldots \overline{\varphi}^{1} f^n \varphi x\right\} = 0$$

for x substitute $\overline{\varphi}^{1} x$, then

$$F\left\{\overline{\varphi}^{1} x, \overline{\varphi}^{1} f x, \overline{\varphi}^{1} f^2 x, \ldots \overline{\varphi}^{1} f^n x\right\} = 0 \qquad (a)$$

which is an equation of the first order relative to $\overline{\varphi}^{1}$ and may be solved by the methods in the beginning of this Paper, or, by means of the method given by Mr. Herschel, to which we have already alluded.

With respect to the function f it is arbitrary, there are however, some observations respecting it, which require notice; as without an attention to them we might fall into error. In the first place, it is evident, that we must not

make fx equal to x, for in this case we at once determine ψx to be equal to x, which is not always true.

The same observation may be made with respect to making $f^n x = x$, for in this case $\psi^1 x = x$, $\psi^2 x = \psi x$, $\psi^3 x = x$, $\psi^4 x = \psi x$, and we in fact by assuming this value for f determine ψ from the equation

$$F\left\{x, \psi x, x, \psi x, x, \&c.\right\} = 0$$

The same objection does not hold when we make $f^2 x = x$, though this considerably limits the generality of the solution; apparently the most eligible mode of determining f is from the equation $f^{n+1} x = x$, for in this case supposing we are acquainted with a particular solution of (a) containing any number of arbitrary constants, such as

$$\varphi^{-1} x = A\left\{x, a, b, c, \&c.\right\}$$

we may substitute for each of these constants an arbitrary function, such as

$$\overline{\chi}\left\{x, fx, f^2 x, \ldots f^n x\right\}$$

for it is evident this function does not change when we substitute $f^n x$ for x.

But this form of f is not always correct, it may be inconsistent with the original equation. In fact, the only value we can assign to f which shall not in some measure limit the generality of the Problem, is to suppose a particular solution of the given equation.

As an example of this method take the following problem.

Required the nature of the curve ADE such that taking any abscissa AB and corresponding ordinate DB: if the abscissa AC be taken, equal to DB and the ordinate EC be drawn, then the rectangle under the two ordinates shall be equal to the square of the first abscissa let the equation of the curve be $y = \psi x$ then $AB = x \quad DB = y = \psi x \quad AC = DB$,

and $$EC = \bar{\psi}(AC) = \psi(DB) = \psi y = \psi\psi x$$

the given condition is therefore

$$\psi^2 x \times \psi x = x^2$$

making the usual substitution of $\psi x = \bar{\varphi}' f \varphi x$ it becomes

$$\bar{\varphi}' f^2 \varphi x \times \bar{\varphi}' f \varphi x = x^2$$

putting $\bar{\varphi}' x$ for x we have

$$\bar{\varphi}' f^2 x \times \bar{\varphi}' f x = (\bar{\varphi}' x)^2$$

Assume $$\bar{\varphi}' x = \chi(x, fx, f^2 x)$$

the equation then becomes

$$\bar{\chi}\{f^2 x, f^3 x, f^4 x\} \times \bar{\chi}\{fx, fx^2, fx^3\} = [\bar{\chi}\{x, fx, f^2 x\}]^2$$

If now f be determined from the equation

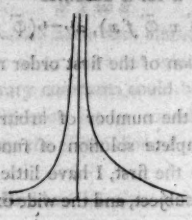
$$f^3 x = x$$

we have $f^4 x = fx$, and the equation becomes identical, hence $\bar{\varphi}'$ and f being found, we may determine ψ.

One of the simplest curves which satisfies the equation is

$$yx^2 = a^3$$

a line of the third order, the 69th in NEWTON's arrangement, its form is



PROBLEM XX.

Required the general solution of the equation

$$F\left\{x, \psi x, \psi^{2} \alpha x, \psi^{3} \beta x, \ldots \psi^{n+1} x\right\} = 0$$

Reduce this by Problem XVIII. to the form of

$$F\left\{x, \psi x, \psi^{2} x, \ldots \psi^{n} x\right\} = 0$$

in which shape it may be solved by the preceding problem.

PROBLEM XXI.

Given the equation

$$F\left\{x, \psi x, \psi\alpha\,(x, \psi\beta x)\right\} = 0$$

required its solution.

Assume $$\alpha\,(x, \psi x) = \overline{\varphi}\, f \varphi x \qquad (a)$$

hence $$\psi x = \alpha^{1,-1}\,(x, \overline{\varphi}\, f \varphi x) \qquad (b)$$

In the left side of this equation, put for x the left side of (a) and in the right side, put instead of x the right side of (a) the result is

$$\psi\alpha\,(x, \psi x) = \alpha^{1,-1}\,(\overline{\varphi}\, f \varphi x, \overline{\varphi}\, f^{2} \varphi x)$$

substituting this in the original equation, the result is

$$F\left\{x, \alpha^{1,-1}\,(x, \overline{\varphi}\, f \varphi x), \alpha^{1,-1}(\overline{\varphi}\, f \varphi x, \overline{\varphi}\, f^{2} \varphi x,)\right\} = 0$$

which by putting $\overline{\varphi}\, x$ for x becomes

$$F\left\{\overline{\varphi}\, x, \alpha^{1,-1}\,(\overline{\varphi}\, x, \overline{\varphi}\, f x), \alpha^{1,-1,}(\overline{\varphi}\, f x, \overline{\varphi}\, f^{2} x)\right\} = 0$$

this being an equation of the first order relative to $\overline{\varphi}^{1}$ may be solved as above.

With respect to the number of arbitrary functions which enter into the complete solution of functional equations of higher orders than the first, I have little at present to offer; the difficulty of the subject, and the wide extent of the enquiries

to which it would lead, induce me to postpone it until I have more time for the consideration. The following remarks may suffice for the present to point out some of its difficulties and the mode of enquiry.

If
$$\psi x = f_1\{x, a, b, \&c.\} \qquad (1)$$
$$\psi^2 x = f_2\{x, a, b, \&c.\} \qquad (2)$$
$$\psi^3 x = f_3\{x, a, b, \&c.\} \qquad (3)$$
$$\&c. \qquad \&c.$$
$$\psi^n x = f_n\{x, a, b, \&c.\} \qquad (n)$$

From this by eliminating $n-1$ of the arbitrary constants $a, b,$ &c. we have an equation of the form

$$F\{x, \psi x, \psi^2 x, \ldots \psi^n x\} = 0 \qquad (a)$$

and it might possibly be concluded that equation (1) containing $n-1$ arbitrary constants is the general solution of this last equation: but this is by no means the case. In the first place between the two equations (1) and (2), more than one arbitrary constant may be eliminated, thus let

$$\psi x = \frac{a-x}{1-bx}$$

from which we find

$$\psi^2 x = x$$

the two quantities a and b have been eliminated, and it is possible to select a value of ψx, between which and $\psi^2 x$ an infinite number of arbitrary constants could be eliminated.

But waving this objection let us consider the case of (a) which is deduced from the elimination of $n-1$ arbitrary functions.

We have seen in Problem VI. that a function of the first

order may satisfy any number of conditions (which are not contradictory) simultaneously; and there appears no reason for denying this property to those of higher orders.

If now we consider the symmetrical function

$$\overline{\chi}\left\{x, \psi x, \psi^2 x, \ldots \psi^v x\right\}$$

and if $\psi^{v+1} x = x$. It is evident this function will not change by the substitution of ψx, $\psi^2 x$, &c. or $\psi^v x$ and consequently that a different function similarly constituted may be substituted for each of the arbitrary quantities a, b, c, &c. in (1) which is the solution of the equation

$$\mathrm{F}\left\{x, \psi x, \psi^2 x, \ldots \psi^v x\right\} = 0 \qquad (a)$$

The number denoted by v is arbitrary (it may, however, become determined from some particular circumstances of the equation (a).

Thus we have introduced into an equation of the n^{th} order, an unlimited number of arbitrary functions, each of which contains the function whose determination was sought with all its different orders to an undefined extent.

If we take the particular case of

$$\psi x = a - x$$
$$\psi^2 x = x$$

v must be unity, and a general solution is

$$\psi x = \overline{\chi}\left\{x, \psi x\right\} - x$$

taking another solution

$$\psi x = \frac{a-x}{1-bx}, \qquad \psi^2 x = x$$

and

$$\psi x = \frac{\overline{\chi}(x, \psi x) - x}{1 - x\,\overline{\chi}(x, \psi x)}$$

If $\psi^v x = x$ $\quad v = n - 1$, and supposing $\psi x = f(x, a, b, \&c.)$ any particular solution we have for the general one

$$\psi x = f\left\{x, \bar{\chi}(x, \psi x, .. \psi^{n-1} x), \bar{\underset{1}{\chi}}(x, \psi x, .. \psi^{n-1} x) \&c.\right\}$$

from which equation of the $\overline{n-1}^{\text{th}}$ degree ψ must be found.

When we apply these considerations to functional equations of many variables, other and even greater difficulties present themselves; the first step in that direction must be an improvement in the notation.

Since the above was written, I have bestowed some attention on functional equations involving two or more variables, and I have met with considerable success: I am in possession of methods which give the general solution of equations of all orders, and even of those which contain symmetrical functions. I have also discovered a new and direct method of treating functional equations of the first order, and of any number of variables, and this new method I have applied to the solution of differential and even of partial differential functional equations.

XXIV. *Some additional experiments and observations on the relation which subsists between the nervous and sanguiferous systems.* By A. P. Wilson Philip, *Physician in Worcester.* *Communicated by* T. Andrew Knight, *Esq. F. R. S.*

Read June 15, 1815.

In a paper which I had the honour to lay before the Royal Society, I observed that M. le Gallois founds his explanation of many of the phenomena which he describes in his Treatise, *Sur la Principe de la Vie*, &c. on the supposition that the circulation nearly ceases in any part when that portion of the spinal marrow from which it receives its nerves is destroyed. The accuracy of this supposition many circumstances led me to question. It is easy to subject it to the test of experiment.

Exp. 1. The spinal marrow of a frog was destroyed by moving, in various directions, a wire introduced into the spine by a hole made in the lowest part of it, and passed up into the brain. The animal was immediately deprived of sensibility and voluntary motion, and appeared to be quite dead. After it had lain in this state for several minutes, part of the web of one of the hind legs being brought into the field of a microscope, the blood was seen circulating in it as rapidly as in the web of a healthy frog. In making such experiments it is necessary to be aware, that handling and stretching the web tends to impair the vigour of the circulation in it. If this experiment is objected to on account of its being made on

an animal of cold blood, I may refer to the seventh and eighth experiments related in the paper above alluded to, in which the carotid and femoral arteries were found beating and performing the circulation after the spinal marrow had been wholly destroyed.

The labours of M. LE GALLOIS, by ascertaining some facts of great importance, while others immediately connected with them escaped his observation, have involved the subject in such seeming contradictions as, at first view, to have persuaded me that some of his experiments were inaccurate. On repeating many of them, however, I was convinced of their accuracy. In some the destruction of the cervical part of the spinal marrow immediately destroyed the function of the heart; yet in others the destruction, in a different way, of the same, or a larger portion of the spinal marrow, little affected it. In some, the greater part of the spinal marrow was destroyed without destroying the function of the heart; yet in others, after the spinal marrow had been divided, he found the function of the heart destroyed by the destruction of either half.

It was the confusion arising from these, and similar difficulties, that occasioned him to observe that he had almost as many results as experiments, and that he had resolved to abandon the investigation, when his explanation of the first of the foregoing difficulties, founded on the supposition which suggested the above experiment, presented itself to him. Had it occurred to him to compare this supposition with the latter difficulty, he would have doubted its accuracy.

The seeming contradictions which appear in the experiments of M. LE GALLOIS cannot be reconciled, except on prin-

ciples different from those hitherto assumed by physiologists. What these principles are, I have endeavoured to ascertain in the paper just mentioned. One part of the subject I left untouched, as it seemed at first sight to open too extensive a field of enquiry. It was evident in making the experiments related in that paper, that the laws which regulate the effects of stimuli applied to the brain and spinal marrow on the muscles of voluntary, and on those of involuntary motion, are very different. The following experiments point out more precisely in what this difference consists.

Exp. 2. Part of the cranium of a rabbit was removed, and a wire passed in various directions through the brain. I could not in this way in the least affect the muscles of voluntary motion, except when I made the wire approach those parts of the brain from which the spinal marrow and nerves originate. The muscles of voluntary motion were then thrown into violent spasms. I sliced off the whole of the upper and anterior part of the brain without affecting the muscles of voluntary motion. The knife only excited their action when it approached the source of the nerves.

Having deprived another rabbit of sensibility and voluntary motion by a blow on the occiput, that I might be enabled to judge of the effects which a stimulus applied to the brain would produce on the heart, I removed part of the cranium and laid open the thorax. The heart was found beating regularly. By passing a wire through the brain in any direction, the beats of the heart were accelerated and rendered stronger. I could not perceive that this effect was produced more powerfully when the wire was directed towards the source of the nerves, than when any other direction was given to it, provided it passed

through an equal portion of the brain. When an instrument was merely pressed gently on the surface of the brain, the effect was similar. When a pair of scissars, or any other thing of larger bulk than the wire was passed into the brain, the effect on the heart was greater than from the wire. It was still greater when the brain was wounded rapidly in many directions.

Exp. 3. Part of the cranium of a rabbit was removed, and after passing a knife through the brain in various directions towards the origin of the nerves, which excited the strongest spasms in the muscles of voluntary motion, the blood being absorbed by a sponge, I applied strong spirit of wine to the surface of the brain, and dropt it into the cuts, without at all affecting the muscles of voluntary motion. The upper part of the brain was then wholly removed, and the space filled with strong spirit of wine, but no spasms were excited in the muscles of voluntary motion.

Another rabbit was deprived of sensibility and voluntary motion by a blow on the occiput. Part of the cranium was then removed, the thorax laid open, and the heart found beating regularly. Spirit of wine was now applied to the surface of the brain, by which the frequency and force of the heart's beats were immediately increased. Several cuts were then made in the brain, and the spirit of wine dropt into them, by which the action of the heart was increased in a much greater degree. Spirit of wine increased the action of the heart more than any mechanical injury, which never produced the strong action in this organ, that it does in the muscles of voluntary motion.

This experiment was repeated with a watery infusion of

opium instead of spirit of wine; the result was in all respects the same, except that the action of the heart was less increased than by the spirit of wine.

Under the term brain, I mean to include the cerebellum as well as cerebrum. From many trials on rabbits made to ascertain the point, I could not perceive that the heart is more or less affected either by chemical or mechanical stimuli applied to the cerebellum than to the cerebrum; nor are the muscles of voluntary motion affected by wounding the cerebellum, except we approach the source of the spinal marrow and nerves. In some of my experiments, I thought that stimuli applied to the cerebellum affected the action of the heart rather more powerfully than when applied to the cerebrum; but this was contradicted by other experiments.

Exp. 4. I repeatedly cut off the head of a rabbit close to the occiput. For some time the trunk and limbs were affected with violent spasms. The cut end of the spinal marrow was so sensible that the slightest touch of a wire, after the spasms had subsided, immediately excited the action of the muscles of voluntary motion. The strongest spirit of wine and watery infusion of opium were applied to it, without producing the least effect on those muscles. The application, however, of stronger chemical stimuli, the nitric and muriatic acids, throw them into powerful contractions.

Having deprived a rabbit of sensation and voluntary motion, in an experiment already laid before the Society, I found that both spirit of wine and a watery infusion of opium applied to the spinal marrow, increase the action of the heart.

Exp. 5. I found both in rabbits and frogs that, after all stimuli applied either to the brain or spinal marrow had ceased to

produce any excitement in the muscles of voluntary motion, both chemical and mechanical stimuli still increased the action of the heart; the former more than the latter.

Exp. 6. I tried, in every possible way, both by mechanical and chemical stimuli, and both before and after the sensibility was destroyed, to excite, through the brain or spinal marrow of rabbits and frogs, any irregular action in the heart which is so readily excited in the muscles of voluntary motion, but could not. Nor could I by sedatives, applied to the nervous system, occasion any irregular action in it. Its action was rendered quicker or slower, more or less frequent, stronger or weaker, but never irregular. The only instance in which irregular action was excited in the heart, was when its power was nearly destroyed by crushing the brain or spinal marrow.

Exp. 7. I found from many trials both on rabbits and frogs, that the excitement of the muscles of voluntary motion took place chiefly at the time the stimulus was applied to the brain or spinal marrow. It was generally necessary to move the instrument; thus applying it to a new surface in order to support the effect. The repeated contractions of the muscles of voluntary motion will sometimes continue, assuming the form of a fit, as long as the instrument remains in the brain, although it be kept as still as the motions of the animal will admit of. The increased action of the heart on the contrary, continued as long as the stimulus, whether chemical or mechanical, was applied, unless it was of a nature to produce the sedative, after the stimulant effect. The sedative effect was so far from being the consequence of the previous excitement, as many physiologists have supposed, that spirit of wine and mechanical stimuli, which produced no sedative

effect, but continued to stimulate the heart as long as they were applied, produced a much greater degree of excitement than tobacco, whose slight stimulant effect was quickly succeeded by a powerfully sedative one.

It appears from these experiments, that chemical stimuli, applied to the nervous system, exert a greater power over the heart than mechanical stimuli, while the latter exert a greater power over the muscles of voluntary motion than chemical stimuli; that both chemical and mechanical stimuli, applied to the nervous system, excite the heart, after they cease to produce any effect on the muscles of voluntary motion; that stimulating every part of the brain and spinal marrow equally affects the action of the heart, while the muscles of voluntary motion are only excited by stimuli applied to those parts of the nervous system from which the spinal marrow and nerves originate; that stimuli applied to the nervous system never excite irregular action of the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion; that their effect on these muscles is felt chiefly on their first application, but continues on the heart as long as the stimulus is applied. These differences in the effects of stimuli applied to the nervous system, on the muscles of voluntary and those of involuntary motion, which seem involved in so much obscurity, must be explained before we can be said to understand the relation which subsists between that system and the heart.

In the following part of this paper, I shall, in the first place, endeavour to trace the causes from which these differences arise; and afterwards to ascertain whether the power of the blood-vessels, like that of the heart, is indepen-

dent of the nervous system, and whether they are directly influenced by that system, or only through the medium of the heart.

It appeared to me probable, from many experiments, that the cause of chemical stimuli, applied to the nervous system, producing a greater effect on the heart than mechanical stimuli do, is, that the former from their nature act on a larger portion of the brain and spinal marrow. If this opinion is correct, the mechanical stimulus will be rendered the most powerful by confining the chemical to a smaller space than the mechanical stimulus occupies.

Exp. 8. Both in frogs and rabbits I applied to various parts of the brain and spinal marrow, and particularly to those parts from which the nerves originate, minute portions of strong spirit of wine, without at all influencing the action of the heart. When these small portions were applied to a great many parts, the heart began to beat more frequently. This of course was much the same thing as at once applying the spirit of wine to a larger part. We have seen in the foregoing experiments, that mechanical stimuli applied to any considerable portion of the nervous system, increase the action of the heart. It appears from the following experiments that we cannot affect the heart by mechanical stimuli confined to any small part either of the brain or spinal marrow.

Exp. 9. In a rabbit deprived of sensibility by a blow on the occiput, I wounded different small parts of the brain with a wire, particularly all those parts near which the nerves of the heart appear chiefly to originate; but could not affect the motion of this organ, while at the same time passing the wire

through any considerable portion of the brain immediately accelerated it.

Exp. 10. I laid open the cervical part of the spine of a rabbit, rendered insensible by a blow on the occiput, and repeatedly passed the wire transversely through the spinal marrow, without being able at all to affect the motion of the heart; but on passing the wire longitudinally, so as to bring it in contact with a larger portion of the spinal marrow, I found the motion of the heart immediately accelerated. On the same principle, when the wire was made to wound many minute portions of the brain and spinal marrow in quick succession, the action of the heart was increased. In another rabbit, I divided the spinal marrow at the occiput without at all affecting the heart.

Mr. CLIFT, in an account of experiments on the Carp, published in the Philosophical Transactions for this year, observes, that on dividing the spinal marrow at the occiput, the action of the heart was greatly accelerated for a few beats; but he divided the spinal marrow while the animal retained the power of the muscles of voluntary motion, which never fail to be called into action by wounding it, and whose action, by increasing the flow of blood, always accelerates the motion of the heart.*

Thus we see that neither chemical nor mechanical stimuli applied to the nervous system, affect the action of the heart,

* It is particularly satisfactory to me that Mr. CLIFT, on repeating my experiment, in which the spinal marrow was destroyed by a hot wire, found the same result in the carp, which I had done in rabbits and frogs. He did not ascertain whether the circulation continued after the destruction of the spinal marrow, but from this occasioning little or no diminution in the action of the heart, we can have little doubt of the continuance of the circulation.

unless they make their impression on a large part of this system. In the various experiments I have related, every part of the nervous system was stimulated individually, without the action of the heart being influenced, and the stimulus being the same, the force with which it acted on this organ, was always proportioned to the extent of surface to which it was applied. I could not find that it was of any importance what part of the brain was stimulated. Even stimulating the surface alone, either mechanically or chemically, immediately increased the action of the heart. The muscles of voluntary motion, on the contrary, we have seen, are wholly insensible to stimuli applied to the nervous system, except near the origin of the nerves. It is remarkable that while a rabbit perfectly retains its sensibility, and expresses great pain on any of the muscles being wounded, it exhibits no expression of pain whatever from the brain being sliced, until the knife approaches the origin of the nerves or spinal marrow.

Another circumstance, which appears to be of great importance in tracing the cause of the different effects of stimuli applied to the nervous system on the muscles of voluntary and involuntary motion, is, that the heart obeys a much less powerful stimulus than the muscles of voluntary motion do. We have seen that only the most powerful chemical stimulus affects them, while all that were tried readily influenced the action of the heart. Mechanical stimuli which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical stimuli, indeed, from their effects on the heart, we should consider the most powerful. But their greater effect on this organ is readily explained, by the influence of stimuli applied

to the nervous system on the heart, being proportioned to the extent of surface to which they are applied. It is evident that the stimulus can be applied to a greater extent of surface in the fluid than in the solid form. When the effect of the mechanical agent is rendered extreme and general on the nervous system, we find its influence on the heart far greater than that of any chemical agent I tried. From experiments I lately laid before the Society, it appears, that suddenly crushing any considerable part of the nervous system instantly destroys the power of the heart.

The conclusions then at which we arrive, are,—that the heart is excited by all stimuli applied to any considerable part of the nervous system, while the muscles of voluntary motion are only excited by intense stimuli applied to certain small parts of this system.

These facts being ascertained, the other differences observed in the effects of stimuli applied to the nervous system, on the heart and muscles of voluntary motion, are easily explained.

Irregular action of a muscle arises from stimuli acting partially, or at intervals, on its nerves, or on the particular part of the brain or spinal marrow, from which its nerves arise. But partial action of a stimulus on the nervous system, we have just seen, is incapable of exciting the heart, and while the stimulus is applied to any part of the nervous system, as all parts of this system seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion when it is moved from place to place in the brain. When the instrument is kept still after it is introduced into the brain, the action of the muscles of voluntary motion often ceases; its merely being in contact with the parts

of the nervous system which excite these muscles, not being sufficient to call them into action. It must bruise or lacerate to produce this effect. As the muscles of voluntary motion feel the impressions made on a very small part of the nervous system only, in proportion as this part is small, the impression must be great to affect them; but the heart, which is influenced through all parts of the nervous system, though not very powerfully through any one, feels all the impressions made on this system, provided they are made on a sufficiently extensive portion of it; thus, as long as the instrument remains in the brain, its stimulant effect on the heart continues.

It is true, that although the heart is only influenced by agents applied to a large portion of the brain, we may conceive them so applied as to produce irregular action in it, and we find that certain irritations of the nervous system have this effect. But it is evident, that the heart not being subject to stimuli whose action is confined to a small portion of this system, and being equally affected through all parts of it, must render it much less subject to irregular action; which may be one of the final causes of this organ, whose regular action is of such importance in the animal economy, being made subject to the whole, and not to any one part of the nervous system; and readily accounts for our not being able to produce irregular action in it, in the above experiments.

What has been said also explains why those, who have endeavoured to influence the heart by stimulating its nerves or the parts of the brain from which they seem chiefly to originate, have failed. When indeed the connection of the nerves of the heart is considered, it will be found to derive its nervous influence from every part of the nervous system, and not very

remarkably from any one part, a circumstance which particularly corresponds with the result of the foregoing experiments.

From the same facts we explain, why the heart is stimulated through the nervous system after the power of this system is so far weakened as no longer to convey the effect of the stimulus to the muscles of voluntary motion. As these obey stimuli applied to only one part of the nervous system, if the change in this part is not strong enough to produce the effect, it cannot be assisted by any other. Thus I have found by experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, because no one part suffers greatly, but it produces a great effect on the heart, because it feels the sum of all the impressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart.

It appears from the foregoing experiments, that the heart is influenced by every part of the nervous system; and in a former paper I pointed out why we have reason to believe that the intestines obey the same laws with the heart, although this cannot be so directly proved. From the situation of the ganglia compared with the whole of the experiments here alluded to, I think we cannot help believing, that their office is to combine the influence of the various parts of the nervous system, from which they receive nerves, and to send off nerves endowed with the combined influence of those parts.

Without some such means, it would be difficult to conceive how any organ should be influenced by every part of the nervous system. We cannot suppose that it receives nerves from every part of this system. Indeed we know, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the nervous system, others only by certain small parts of it. In the latter instances, we see nerves going from these small parts directly to the parts influenced. In the former instances, namely, where it is found that the part is influenced by all parts of the nervous system, we see no nerve going directly from any part of this system to the parts influenced; but we see these parts receiving nerves from ganglia, to which nerves from every part of this system are sent. It is therefore evident from direct experiment, that the nerves issuing from ganglia convey the influence of all the nerves which terminate in them, to the parts to which they send nerves; and consequently that this is one use of the ganglia; nor does there seem any reason to induce us to believe, that they have any other use. Thus it would appear, that the ganglia and nervous filaments connecting them, which have been called the great sympathetic nerve, are, if I may be allowed the expression, a channel of nervous influence flowing from every part of the brain and spinal marrow, from which those organs are supplied, which are subjected to the influence of the whole nervous system; those subjected to any particular part of this system, being supplied directly from that part. This view of the subject is consistent with the observations of anatomists, who remark that the great sympathetic has by no means the character of a nerve. Nothing surely can be

more different than this string of ganglia and a nerve, such as it passes directly from the brain and spinal marrow to the muscles of the trunk or limbs. It may also be worth remarking, that the nerves sent off from ganglia, have a very different appearance from those coming directly from the brain and spinal marrow. *Recherches Physiques sur la vie et la mort, &c. par M. Bichat.*

The question has been much agitated, why the will has no influence over the muscles of involuntary motion. It has been supposed that the ganglia intercept its influence, but we see in the above experiments, that the ganglia do not intercept the influence of either stimuli or sedatives applied to the nervous system. We can be at no loss to account for our want of power over these muscles, when we consider, that in their ordinary action, they obey stimuli over which we have no influence; and that, at all times, we neither see nor are otherwise conscious of their motions, and consequently cannot direct them.

I have endeavoured by the following experiments to ascertain, whether the power of the blood vessels is as independent of the nervous system, as that of the heart; and whether this system possesses over them the same kind of influence, as over the heart.

These experiments were made on the capillaries of the frog, which, from the extent and transparency of the web of its hind feet, and from its great tenacity of life, appeared the best subject for such experiments. It has been questioned, how far inferences drawn from experiments made on cold blooded animals, can be supposed to apply to those of

warm blood. Both FONTANA and Dr. MONRO observe, that in their experiments they found the system of both obeying the same laws. The experiments I have had occasion to lay before the Society, tend to confirm this observation; and I may say the same of all the experiments I have made on both sets of animals. There are certain circumstances in which they evidently differ, in all others they seem to agree. The following experiments ought not to be unnecessarily repeated, and as there is no part of the warm blooded animal on which they could be satisfactorily made except the mesentery, they would be attended with much greater suffering in this, than in the cold blooded animal. Some of them, from the warm blooded animal being less tenacious of life, could not be performed on it.

Exp. 11. A strong ligature was thrown round the neck of a frog, and the head cut off without any loss of blood; much loss of blood immediately destroys the circulation in the extremities. The spinal marrow was then destroyed by a wire. On bringing the web of one of the hind legs before the microscope, the circulation in it was found to be vigorous, and continued so for many minutes. This experiment was repeated with the same result.

It appears from this experiment, that the blood vessels retain their power after the nervous system is wholly destroyed. In order to ascertain, how far the vessels can be stimulated through the nervous system, independently of any action of this system on the heart, it is necessary in the first place to determine, whether the vessels can support the motion of the blood independently of the heart.

Exp. 12. A ligature was thrown round all the vessels

attached to the heart of a frog, and the heart was then cut out. On bringing the web of one of the hind-legs into the field of the microscope, the circulation in it was found to be vigorous, and continued so for many minutes; at length gradually becoming more languid.

In endeavouring to proceed farther, I found much difficulty. It was not only necessary, in order to ascertain the effect of stimuli applied to the nervous system on the vessels of the web, to remove the heart, and to lay open the cranium, but also to prevent the voluntary motions of the animal, which continually occurred, and never failed to accelerate the motion of the blood in the web.

Exp. 13. A frog was deprived of sensibility and voluntary motion, by the upper parts of the body being immersed in laudanum; part of the cranium was then removed, after a ligature had been thrown round the neck to prevent loss of blood. The thorax was now opened, and all the vessels attached to the heart included in a ligature. But notwithstanding this experiment was repeatedly performed with the greatest care, the circulation by all these preparatory means was so enfeebled, that although the blood still moved in the web, it was in so irregular and uncertain a way, that I never could arrive at any positive conclusion respecting the effect of the stimulus applied to the brain. After many fruitless attempts, therefore, I abandoned this mode of making the experiment.

Although the action both of the heart and the muscles of voluntary motion so influence the effect of stimuli applied to the brain, on the circulation in the foot, that, without wholly preventing the effect of both, no conclusion can be drawn, it is evident that the action of the latter cannot increase the effect of

sedatives; and the sedative lessening the power of the heart will not affect the result of the experiment, if it be made on the web of the frog. We have just seen, that the total ceasing of the action of the heart, does not for a considerable time affect the circulation in it. The following experiment appears to be decisive of the effect of the sedative, and of the stimulus, as far as this can be decisive, the action of the heart remaining. It is evident that the action of either stimulus or sedative is equally conclusive respecting the direct influence of the nervous system on the blood vessels.

Exp. 14. Part of the cranium of a frog was removed, the web of one of the hind legs brought into the field of the microscope, and the circulation in it observed. The animal was now rendered insensible by the immersion of the other hind leg in laudanum. The insensibility did not in the least affect the circulation in the web before the microscope. Spirit of wine was then applied to the brain with an evident increase of the velocity of the blood in the web. The same effect was produced in a less degree by watery solutions of opium and tobacco. After the tobacco had been applied for about half a minute, the motion of the blood was much less frequent than before its application. On washing off the tobacco the velocity of the blood increased, and was again lessened on applying it. This was repeated several times with the same effects. The following way of performing the experiment, is equally conclusive.

Exp. 15. A frog was rendered nearly insensible by having its back immersed in laudanum. A ligature was then thrown round the neck to prevent loss of blood, part of the cranium

removed, the web of one of the hind legs brought before the microscope, and the circulation in it, which was rapid, observed. A strong infusion of tobacco was then applied to the brain, with the effect of at first rendering the circulation more rapid. In about half a minute it became more languid, and soon stopped altogether. On the infusion of tobacco being washed off, the circulation returned and regained considerable vigour. The tobacco was several times applied to the brain and washed off, with the same effects. I may observe, that when the circulation in the web had almost ceased after the tobacco had been washed off, its velocity was immediately increased on applying spirit of wine to the brain.

Exp. 16. Analogous to what I had occasion to observe respecting the heart, I could never, either by chemical or mechanical agents, excite any irregular action in the blood vessels. Their action was only rendered more or less powerful.

The irregular appearances in the circulation in the web of a frog's foot, mentioned by Dr. Thompson, Professor of Military Surgery in the University of Edinburgh, in his Lectures on Inflammation lately published, and which he ascribes to inflammation, may be observed in any case, if the vessels be at all compressed in applying the foot to the microscope; and although they are not compressed, these appearances very generally occur when the circulation begins to fail. The blood will then stop and go on at intervals, and move backwards and forwards in the same vessel. I have often watched the capillaries from the commencement of inflammation to its greatest height, when the part is about wholly to lose its vital power, in the mesentery of a rabbit, the web of a frog's

foot, and the fins of fishes, without perceiving the least tendency to this irregular motion when the part viewed was so applied to the microscope as not to compress any of its vessels.*

The power of the blood vessels, like that of the heart, is capable of being directly destroyed through the medium of the nervous system.

Exp. 17. The web of one of the hind legs of a frog was brought into the field of the microscope, and while Mr. HASTINGS, who was good enough to assist me in this and the 14th experiment, observed the circulation, which was vigorous, I crushed the brain by the blow of a hammer. The vessels of the web instantly lost their power, the circulation ceasing. In a short time the blood again began to move, but with less force. This experiment was repeated with the same result. If the brain is not completely crushed, the blow increases the rapidity of the circulation in the web.

Exp. 18. The spine of a frog was laid open at the lower end, and a wire of nearly the same dimensions with its cavity, forced through it, as in M. LE GALLOIS's experiments. The web of one of the hind legs was then brought into the field of the microscope, and the circulation in it was found to have wholly ceased. In another frog, as we have seen,† the spinal marrow was destroyed by the introduction, in the same way, of a wire much smaller than the cavity of the spine, and by its being moved in various directions. The frog soon appeared

* An account of these experiments is published in the introduction to the second part of my Treatise on Febrile Diseases, and a plate given representing the state of the vessels in the different stages of inflammation.

† See Experiment 1.

to be quite dead, but the circulation in the web was found to be vigorous.

From the foregoing experiments and observations, it appears,

1. That the laws which regulate the effects of stimuli, applied to the nervous system, on the muscles of voluntary and involuntary motion, are different. Exp. 2, 3, 4, 5, 6, 7.

2. That both mechanical and chemical stimuli, applied to any considerable portion of the nervous system, increase the action of the heart. Exp. 2, 3.

3. That neither mechanical nor chemical stimuli applied to the nervous system, excite the muscles of voluntary motion, unless they are applied near to the origin of the nerves and spinal marrow. Exp. 2, 3, 4.

4. That mechanical stimuli applied to the nervous system, are better fitted to excite the muscles of voluntary motion, and chemical stimuli, those of involuntary motion. Exp. 2, 3, 4.

5. That after all stimuli, applied to the nervous system, fail to excite the muscles of voluntary motion, both mechanical and chemical stimuli, so applied, still excite the heart. Exp. 5.

6. That both mechanical and chemical stimuli applied to the nervous system, excite irregular action in the muscles of voluntary motion. Exp. 2, 3, &c.

7. That neither excite irregular action in the heart, nor is its action rendered irregular by sedatives, unless a blow which crushes the brain be regarded as a sedative. Exp. 6.

8. That the excitement of the muscles of voluntary motion takes place chiefly at the moment at which the stimulus is

applied to the nervous system, that of the heart continues as long as the stimulus is applied. Exp. 7.

9. That the muscles of voluntary motion are excited by stimuli applied to very minute parts of the nervous system. Exp. 2, 3, 4.

10. That no stimulus applied to any minute part of the nervous system, can excite the heart. Exp. 8, 9, 10.

11. That the heart obeys a much less powerful stimulus than the muscles of voluntary motion. Exp. 3, 4, &c. and observations after Exp. 10.

12. That the facts expressed in the three last sentences 9, 10, 11, afford an easy explanation of those expressed in the preceding sentences. See the observations after Exp. 10.

13. That the power of the blood vessels, like that of the heart, is independent of the nervous system. Exp. 1, 11.

14. That the blood vessels can support the motion of the blood after the heart is removed. Exp. 12.

15. That the blood vessels are directly influenced through the nervous system in the same way that the heart is. Exp. 14, 15.

16. That analogous to what we observe in the heart, no stimulus or sedative applied to the nervous system, excites irregular action in the blood vessels. Exp. 16.

17. That the power of the blood vessels, like that of the heart, may be destroyed through the nervous system. Exp. 17, 18.

18. That the office of the ganglia is to combine the influence of the various parts of the nervous system, from which

they receive nerves, and to send off nerves endowed with the combined influence of those parts.

19. That the will has no influence over the muscles of involuntary motion, because in their ordinary action they obey stimuli, over which we have no influence, and because at all times we neither see, nor are otherwise conscious of, their motions; and consequently cannot direct them.

20. That we have reason to believe that the division of the encephalon into the cerebrum and cerebellum, relates to the sensorial functions, since it does not appear to relate to the nervous functions, the muscles of voluntary and those of involuntary motion being influenced in the same way by both.

21. That the sedative effect is not the consequence of previous excitement, but the effect of a certain class of agents.

Exp. 7.

ADDITIONS AND CORRECTIONS.

Page 99, line 5, *for* Protagoras, *read* Protogenes
186, last line, *for* southerly, *read* northerly
204, line 4, *for* heat, *read* iodine
272, line 17, for *about* 10°, read *between* 12° *and* 14°
275, line 18, *for* 39°, *read* 37° 30′
277, line 17, *for* nearly $\frac{1}{180}$, *read* less than $\frac{1}{170}$
Id. lines 21 and 23, *for* 141°, *read* 141° 53′
278, line 12, at the word crystallized, insert the following as a note: *The interrupting stratum is crystallized in a different manner from the rest of the rhomboid. The position of its axes with respect to those of the rhomboid, and the singular optical phenomena which arise from this cause, will be described in another paper.*
288, Note, line 4, *for* parallel, *read* perpendicular

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	European Magazine for November.	The Editor.
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	Annals of Philosophy. No. 25.	Dr. Thomas Thomson.
	The European Magazine for December.	The Editor.
	The Naturalist's Miscellany. No. 12.	Dr. W. E. Leach.
19.	Elements of the Philosophy of the Human Mind, by Dugald Stewart. 2 Vols. Edinburgh, 1814. Vol. I. 8°. Vol. II. 4°.	Dugald Stewart, Esq.
	Philosophical Essays, by Dugald Stewart. Edinburgh, 1810. 4°	———
	Biographical Memoirs of Dr. Adam Smith, Dr. William Robertson, and Dr. Thomas Reid, by Dugald Stewart. Edinburgh, 1811. 4°	———
	Notitia Collectionis insignis Vermium intestinalium ab Administratione Reg. Cæs. Musei His. Nat. Viennensis. Vienna, 1811. 4° 3 copies.	The Society of Natural History at Vienna.
	An Electrical Pile, made by Professor Zamboni at Verona.	James Alexis Doxat, Esq.
Feb. 2.	The Speech of Dr. D. Antonio Joseph Ruiz de Padron, relative to the Inquisition.	
	Bread and Bulls, an Apologetical Oration on the flourishing State of Spain in the Reign of King Charles the IV. by Don Gaspar de Jovellanos. Mediterranean, 1813. 4°	Edward Hawke Locker, Esq.
	Considerations on the Protection required by British Agriculture, and on the Influence of the Price of Corn on exportable Productions, by William Jacob, F.R.S. London, 1814. 8°	William Jacob, Esq.
	The Zoological Miscellany. No. 13.	Dr. W. E. Leach.

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Feb. 2.	Annals of Philosophy. No. 26.	Dr. Thomas Thomson.
	The Philosophical Magazine for January.	Mr. Alexander Tilloch.
	The Monthly Review for January, and Appendix to Vol. 75.	Mr. Griffiths.
	The European Magazine for January.	The Editor.
9.	Recherches expérimentales et mathématiques sur les Mouvemens des Molécules de la Lumière autour de leur Centre de Gravité, par M. Biot. Paris. 1814. 4°	M. Biot.
	Astronomisches Jahrbuch für das Jahr 1817. Berlin, 1814. 8°	Prof. E. Bode.
16.	Religious Establishment, a Sermon preached in the Cathedral Church of St. Peter, York, by the Rev. Francis Wrangham, A.M. York. 1814. 4°	Rev. F. Wrangham.
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	Das Licht vom Orient. Nurnberg, 1808. 8°	———
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	Denkschriften der Königlichen Akademie der Wissenschaften zu München für das Jahr. 1813. Munich, 1814. 4°	The Royal Academy of Sciences at Munich.
	Mémoires de l'Académie Impériale des Sciences de St. Petersbourg. Tome IV. St. Petersburg. 1814. 4°	The Imperial Academy of Sciences at St. Petersburgh.
Mar. 2.	Annals of Philosophy. No. 27.	Dr. Thomas Thomson.
	Philosophical Magazine for February.	Mr. Alexander Tilloch.
	The European Magazine for February.	The Editor.
	The Zoological Miscellany. No. 14.	Dr. W. E. Leach.
April 6.	A Geological Essay on the imperfect Evidence in support of a Theory of the Earth, deducible either from its general Structure, or from the Changes produced on its Surface by the Operation of existing Causes, by J. Kidd, M.D. Oxford, 1815. 8°	Dr. J. Kidd.
	Part of the Introductory Lecture for the year 1815, exhibiting some of Mr. Hunter's Opinions respecting Diseases, delivered before the Royal College of Surgeons, London, by John Abernethy. London, 1815. 8°	John Abernethy, Esq.

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	European Magazine for March.	The Editor.
	The Zoological Miscellany, No. 15.	Dr. W. E. Leach.
	Mathematical Tables by the Rev. Mr. Allayne Barker. London. 1814. 12°	Rev. Mr. Barker.
May 4.	Histoire critique de l'Etablissement des Colonies Grecques, par M. Raoul Rochette, Paris, 1815. 4 Vols. 8°	M. Raoul Rochette.
	A Treatise Explanatory of the Principles constituting the Practice and Theory of the Violoncello, by John Macdonald, Esq. London, 1811. fol.	John Macdonald, Esq.
	An Appendix to ditto. London, 1815. fol.	
	Travels in the Ionian Isles, Albania, Thessaly, Macedonia, &c. by Henry Holland, M. D. London, 1815. 4°	Dr. Holland.
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	The Monthly Review for April.	Mr. Griffiths.
	Annals of Philosophy, No. 29.	Dr. Thomson.
	The Philosophical Magazine for April.	Mr. A. Tilloch.
	The European Magazine for April.	The Editor.
11.	Journal des Mines, No. 215, and 216.	Conseil des Mines de France.
	Recherches expérimentales anatomiques, chymiques, &c. sur la Physique des Animaux Mammifères hybernans, par M. I. A Saissy, Paris, 1808. 8°	M. Saissy.
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	Archaeologiae Americanae Telluris Collectanea et Specimina, part 1st, Philadelphia, 1814. 8°	Professor Barton.
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	Journal des Mines, No. 207—213.	Conseil des Mines de France.
	Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, Vol. 2. London. 8°	Society for the Improvement of Medical and Chirurgical Knowledge.
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	Philosophical Magazine for May.	Mr. Alex. Tilloch.
	Annals of Philosophy, No. 30.	Dr. Thos. Thomson.
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	Memoirs of the Caledonian Horticultural Society, Vol. I. Edinburgh, 1814. 8°	The Caledonian Horticultural Society.
15.	An Appendix to a Philosophical Treatise on the Hereditary Peculiarities of the Human Race, by Joseph Adams, M. D. London, 1815. 8°	Dr. Joseph Adams.

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